THURSDAY, OCTOBER 16, 1890.

ANALYTICAL MECHANICS.

A Treatise on Analytical Mechanics. By Bartholomew Price, M.A., F.R.S., F.R.A.S., Sedleian Professor of Natural Philosophy, Oxford. Vol. II. Dynamics of a Material System. Second Edition. (Oxford: Clarendon Press, 1889.)

A SECOND title-page describes the present work as volume iv. of "A Treatise on Infinitesimal Calculus," so that Prof. Bartholomew Price's well-known four volumes may be taken to represent the curriculum of the Infinitesimal Calculus and its applications for the mathematical student at Oxford.

To one accustomed to the style of the text-books in use at Cambridge, the contrast is very striking; the Oxford student is much to be envied for the leisurely and luxuriant way in which the subject is here presented, which follows on the lines of Lagrange and Laplace, and utilizes all the resources of analysis. A student who has been through the present work will be prepared to appreciate the purely geometrical form in which the Newtonian methods, insisted upon at Cambridge, would present some of the theorems in a more fundamental and incisive form; but to our mind the Cambridge system is inferior, which ostensibly insists on the purely geometrical methods before allowing the student to make use of the power of analysis.

Although Newton claims to be one of the inventors of this Calculus, and must have employed its methods in the discovery of his theorems, yet he carefully covered up all traces of the analytical scaffolding, and exhibited a theorem in the "Principia," like a Greek temple, in pure geometrical form.

His influence on his successors was too great when they attempted to follow in the same lines, with the consequence that our insular school of mathematics lagged hopelessly in rear of Continental progress.

Although prescribed as the text-book at Cambridge, the "Principia" is not studied in the original Latin, as Newton wrote it, from one end to the other; but the student makes use of commentaries and selections, which, in accordance with the regulations, he professes to appreciate and apply, before knowing even by sight the supposed mystifying symbols of dy/dx and $\int ydx$.

We might as well send out our soldiers armed with muzzling loading guns, or even bows and arrows, to meet a continental army equipped with the most recent inventions of magazine rifles and breech-loading artillery.

Thus the late R. A. Proctor could write that, although a wrangler, he knew nothing of the Differential Calculus till some time afterwards, when he had to pick it up of himself; however, by a recent regulation, only passed a few weeks ago, a most stupendous change has been made in the Mathematical Tripos, by prescribing a certain very elementary course of Analytical Geometry and the Calculus in the First Three Days.

At Cambridge the large number of candidates for mathematical honours acts as a check to change; and as the same papers have to serve for such widely different classes as the wranglers and the junior optimes, it may happen that a candidate who merely writes out book-work will beat a better mathematician who is tempted to try the difficult questions.

The number of students in mathematics at Oxford is much smaller, and the standard for honours is higher; so that we can take this treatise on Infinitesimal Calculus and contrast it with the extracts from Newton's "Principia," to illustrate the relative standards.

Under the enthusiastic influence of a Sylvester we may see the mathematical school at Oxford the first in this country, as it was two hundred years ago, in the days of Wren, Wallis, Keill, and the founders of the Royal Society, which had its origin in Oxford.

At the outset of the Dynamics of a Material System in space, it is necessary to discuss a number of theorems in solid geometry on the distribution of principal axes and the associated theorems of confocal quadrics (chapter i.); also the kinematics of a rigid body, involving the composition and revolution of angular velocities, and the transformation of co-ordinate axes (chapter ii.).

The author could simplify the distinction between the two systems of rectangular axes by adopting Maxwell's comparison with the screw, right-handed or left-handed. All specifications of rotation as clock-wise, or counter-clock-wise, are ambiguous; because the direction changes as we pass from one side to the other of the clock face. Standing at dusk about a quarter of a mile from a windmill, nearly in the plane of the sails, it is possible by a slight mental effort to change the apparent direction of rotation, and back again, as often as we please.

The author does not permit himself the use of the elliptic functions; or else he would have found the wonderful chapter i., t. ii., of Halphen's "Fonctions Elliptiques" of great service in giving the representation of the cosines of the angles which a movable straight line or a movable set of three rectangular axes makes with three fixed rectangular axes. Much of the subsequent work on Euler's three angles, the integration of his equations of motion, and of the spherical pendulum, &c., could be completed and the integrations effected by the use of Halphen's formulas.

Dynamics proper is introduced in chapter iii., where D'Alembert's principle is employed to establish the equations of motion of a material system, with the subsequent corollaries of the independence of the motions of translation and rotation, and the principles of the conservation of momentum and energy.

It is more the fashion now to dispense with D'Alembert's principle, and to refer immediately to Newton's third law of motion; still, D'Alembert's principle, although a mere corollary, states the thing in such a way as to lead immediately to the formation of the six equations of motion; and by stating it in such a manner as to reduce all dynamical principles to a statical form—"the reversed effective forces and the impressed forces form a system in equilibrium, the internal cohesive forces (stresses) being in equilibrium among themselves"—it was formerly considered that a simplification was effected.

But Maxwell, in his "Matter and Motion," by considering Newton's third law of motion as merely the definition of a *stress*, has been able to restate all the theorems involved in D'Alembert's principle in a few simple sentences, and in a much more convincing form.

It is a pity that the term *vis viva* has been allowed to remain in this last edition, and that it was not entirely replaced by *kinetic energy*: the contrasted term *vis mortua* has been dead for a long time, and *vis viva* should have followed long ago.

The transformations of the dynamical equations into the Lagrangian and Hamiltonian forms are introduced at rather an early stage; and the subject is resumed in the last chapter x., on "Theoretical Dynamics," written by the late Prof. W. F. Donkin. These transformations are merely analytical illustrations of the change of independent variables, the form of the equations depending on whether we express the kinetic energy in terms of the generalized velocities or the generalized momenta.

A clear and expressive notation, somewhat in the style of that found necessary in Thermodynamics, would make these equations more intelligible and convincing; but in any case, the application to definite problems, especially where the geometrical constraints present any peculiarity, is so difficult and refined, that these equations are dangerous weapons to put into the hands of any but advanced students.

The principles of Least Action and of Least Constraint are also introduced here by the author; interesting verifications are thus afforded of well-known problems; but these principles again would not be employed for choice; and although the author pleads in their favour, we think it should not be forgotten that the principle of Least Action was employed to bolster up the Corpuscular Theory of Light.

Newton's principle of mechanical similitude, in the next section, is, however, of great practical importance, and we see its application in the constantly increasing size of our bridges, ships, and guns. In its particular application to naval architecture, a corollary goes by the name of Froude's law (also enunciated by Reech), which asserts that in similar vessels run at speeds proportional to the square root of the length or the sixth root of the displacements, the resistances are as the displacements; and thus the naval architect is able to infer, from the known performance of a ship or a model, what to expect on a different scale. When we make, in any two similar machines of the same material, the velocities in the ratio of the square root of the linear dimensions, we ensure in this manner that the stress per unit area in the material is the same, and thus the two machines are equally strong; so that this law of corresponding speed is most useful in the practical application of Newton's law of similitude.

A valuable section on Units, No. 9, points out that there are only two systems which need be considered: the British foot-pound-second (F.P.S.) system, and the metric centimetre-gramme-second (C.G.S.) system. The author's numbers for the conversion of one system into the other are not exactly according to the latest determinations; thus it is more accurate to make 1 metre = 39·37079 inches, and 1 foot = 30·4794 centimetres. The metre was originally designed so that the kilometre should be the centesimal minute of latitude, for use in navigation; but taking the sexagesimal minute of latitude as 6080 feet, the Admiralty standard, then the above figures make the length of the earth's quadrant 10,007 kilometres, instead of 10,000, as designed. It has been decided, however, for electrical purposes that 10^9 centimetres should be called a *quadrant*, although about 0·07 per cent. out.

Recent redeterminations of the weight of a metre cube of water, and of the volume of 10 gallons or 100 pounds of water, made with the greatest care, have revealed perceptible discrepancies with former estimates; so that the definition of the kilogramme as a decimetre cube of pure water at its maximum density must be considered a purely academic definition, and not sufficiently precise for legal purposes; the ultimate appeal being to the lump of platinum preserved in the Conservatoire des Arts et Métiers.

A Committee of the British Association is at present engaged in attempting to fill up the gaps in our dynamical terminology: the author introduces the *dyne* and *erg*, due to a former Committee, but not the *kine*, *spoud*, *bole*, and *barad*, recently settled upon as names for the C.G.S. units of velocity, acceleration, momentum, and pressure. The C.G.S. units are too minute for practical purposes, so that electricians now employ the *joule*, of 10^7 ergs, and the *watt* as the volt-ampere, or power doing one joule per second—units based really upon the commercial units of the metre and kilogramme, instead of the centimetre and the gramme. These microscopic units were adopted by the original Committee apparently merely to gratify the fad of making $W = sV$, instead of $1000sV$.

The astronomical unit of mass is defined in § 143; but if it is difficult to measure the volume of a kilogramme of water, the probable error in the determination of this astronomical unit of mass is immensely greater; so that to our mind this unit had better be discarded, and the gravitation constant introduced into the equations, using its provisional value, $10^{-8} \times 6{\cdot}48$ C.G.S. units (Everett, "Units and Physical Constants," § 72).

Chapter iv. discusses the equations of motion of a rigid body expressed in terms of angular velocities and their increments, &c. The author adopts various illustrative methods, but to our mind the simplest procedure is to establish the general equations, with the usual notation $\dot{h}_1 - h_2\theta_3 + h_3\theta_2 = L, \ldots$; and then Euler's equations, &c., follow as particular cases. By adding the terms due to the employment of a movable origin we obtain the form of the Hamiltonian equations required in the discussion of the motion of a body moving in a liquid; and here is a good opportunity for the introduction of Dr. Routh's principle of the Ignoration of Co-ordinates, required to complete the theory of the generalized equations of motion.

Prof. Price could make a very useful book for students of elementary mathematics by taking out and printing separately the part on uniplanar motion and its illustrative examples (chapter v.): the complication of the subject of rigid dynamics is more than doubled when we consider motion in three dimensions; but in two dimensions the subject is within the grasp of most students, who will thus acquire a good working knowledge sufficient for most purposes. At the outset the determination of simple moments of inertia is required, and this involves a knowledge of integration; so that a student, untrained in the Calculus, can make very little headway. It is a pity that the lack of the slight knowledge of integration required for this purpose prevents most of our students from going on to the real study of the pendulum, the motion of the

wheel and wheeled carriage, and of the ballistic pendulum. Prof. Price calls the inventor Captain Robins; but, according to the preface of his "Mathematical Tracts," Robins was of Quaker extraction (like so many other students and inventors of warlike instruments), and his only military employment was as chief engineer of the East India Company, in planning and carrying out their fortifications.

In chapter vi. the rotation of a rigid body about a fixed point is discussed, with applications to the three important problems of motion under no forces with Poinsot's geometrical representation, the motion of the top or gyrostat, and the precession and nutation of the earth's axis. These problems illustrate very strikingly the great increase in complication when we go from plane motion to motion in space. The figure of the herpolhode, on p. 251, shows points of inflexion; but, as the author mentions in § 295, these points of inflexion cannot exist in Poinsot's herpolhode. An elegant geometrical demonstration is given on p. 379 of Sylvester's extension of Poinsot's representation, where confocals to the momental ellipsoid are made to roll upon parallel planes; and now it is possible in certain corresponding herpolhodes for points of inflexion to make their appearance; the analytical and geometrical discussion of this problem has engaged the attention of de Sparre and Hess.

We mentioned at the outset that the author did not permit himself the use of elliptic functions; but apparently he could not resist the temptation of introducing them in the complete solution of Poinsot's motion, the simplicity and elegance of the representation being so great. In the separating case, when the modulus of the elliptic functions becomes unity, the introduction of the corresponding hyperbolic functions would have exhibited an analogous symmetry.

By considering the elliptic functions as defined by plane pendulum motion, some of the results in the motion of the top or gyrostat could have been exhibited by comparison with a plane pendulum; but it must be confessed that the simplicity is not maintained when we investigate the projection of the motion on a horizontal plane, without we introduce functions invented by Hermite, of a higher degree of complication.

In the discussion of precession and nutation, a simplification can be introduced by making use of the observed fact in determining the latitude, that the deviation of the axis of rotation from the axis of figure, although certainly existing, is quite inappreciable in the case of the earth; so that the axes of figure, of rotation, and of angular momentum may be taken as coincident. With this approximation the pole of the earth follows a point 90° in longitude behind the sun or moon with a certain velocity; and now the rest of the calculation of precession and nutation becomes a kinematical problem.

Chapter vii. discusses interesting and important problems of small oscillations and of bodies rolling on each other, *e.g.* of a billiard ball on the table; and chapter viii., on relative motion, is important as showing how far we are justified in applying our dynamical equations to the problems going on around us, considering that they take place on the surface of the earth, which is moving in a complicated manner in space. The corresponding elementary discussion in Maxwell's "Matter and Motion," on the ideas of relative motion, and the modification of the principles of dynamics to make them rigorous, is well worth attention at this point.

The deviation from the vertical of a body let fall down a deep mine, of a projectile from the vertical plane of fire, and the rotation of the plane of oscillation of Foucault's pendulum, are discussed as illustrations of the influence of the earth's rotation in modifying a dynamical question; but considering how slight a disturbing cause, such as a current of air, would be sufficient to mask the effect, we believe that these effects have not yet really been observed.

In Foucault's pendulum a very slight jockeying can make the thing go as we wish; while with artillery fire at long ranges the disturbing cause of deviation or drift quite overpowers any deviation due to the rotation of the earth. Theoretically, Foucault's pendulum, if set swinging in a plane through the rising moon, should continue to follow the moon; and roughly speaking, a shot fired at the rising moon should keep moving in the moving vertical plane through the moon, and would thus fall to one side of its original vertical plane of fire; in a range of twelve miles, and a time of flight of one minute, this deflection would, in the latitude of Shoeburyness, amount to about 71 yards, out of about 1000 yards observed average lateral deviation.

A few simple problems on the vibration of elastic threads and plates are given in chapter ix.; and chapter x., as already mentioned, is occupied by Prof. Donkin's contribution on Theoretical Dynamics.

Throughout the work good collections of illustrative examples are introduced, to test the student in his grasp of the principles given immediately before. If we might make a slight criticism, we should suggest the introduction of some arithmetical exercises on these problems, taken from examples in real life; for, as Sir William Thomson insists, it is from arithmetical applications that the student obtains a real grasp of dynamics; the examples given here only testing his algebraical and geometrical power.

In conclusion, we congratulate the student of mathematics at Oxford on the possession of such an admirable text-book, fully brought up to date in the latest developments.

A. G. GREENHILL.

ANNALS OF THE ROYAL BOTANIC GARDEN, CALCUTTA.

Annals of the Royal Botanic Garden, Calcutta. Vol. I. Appendix—(1) "Some New Species of Ficus from New Guinea," by George King, F.R.S., &c., Superintendent of the Royal Botanic Garden, Calcutta. (2) "On the Phenomena of Fertilization in *Ficus Roxburghii*, Wall," by D. D. Cunningham, F.L.S., &c., Surgeon-Major, Bengal Army. (Bengal Secretariat Press, 1889.)

ABOUT a dozen new species of Ficus are added here to Dr. King's valuable monograph of the figs of the "Indo-Malayan and Chinese countries," which occupies the whole of the first volume of the "Annals." It may be remembered that Dr. King proposed a modified classification of the species of *Ficus*, based upon characters indicating, in his view, the direction of evolution in the genus, beginning with a small group having pseudo-hermaphrodite flowers (*Palæomorphe*),

and ending with another small group (*Neomorphe*) remarkable for having di- or tri-androus male flowers, and the receptacles (fruit) borne in clusters, often very large, on the trunk and branches, sometimes at the very base of the trunk.

Curiously enough, although the other five groups or sections, into which King divides the genus *Ficus*, are all represented among the additional species from New Guinea, neither the oldest nor the newest is; but both are represented there by previously known species, and the *Neomorphe* by some of the most remarkable of the genus. Thus, imperfectly as the flora of New Guinea is known, there are indications of great age and variety. Noteworthy among the species figured in the present work is *Ficus hesperidiiformis*, King, belonging to the section *Urostigma*, which is characterized by having male, female, and "gall-flowers" intermixed in the same receptacles. *Ficus hesperidiiformis* resembles the familiar india-rubber tree, *F. elastica*, but the leaves are larger and the receptacles (fruit) very much larger; the ripe dry ones resembling small oranges, hence the specific name.

Dr. Cunningham's memoir on the fertilization of *Ficus Roxburghii* is an interesting and important contribution to the subject of reproduction, inasmuch as he arrives at some rather startling conclusions with regard to the plant in question.

The relations between certain insects, parasitic in the receptacles of the fig and caprifig, and in various other species of fig, and the fertilization of the flowers, has been investigated in recent times more especially by Dr. G. King, Mr. Fritz Mueller, and Count Solms; and particulars of their results, or conclusions, have been given from time to time in NATURE (vols. xxvii. p. 584, xxxvi. p. 242, and xxxix. p. 246). Nevertheless it may be well to repeat here some of the principal conditions and phenomena of the flowers of figs. In the first place it may be noted for unbotanical readers that the flowers of figs are very small and crowded all over the interior of the receptacle or fruit. Further, that the wall or substance of the receptacle is continuous and closed, except at the apex, where it is provided with a number of closely overlapping scales, rendering ingress, and egress, without eating its way, impossible to any but a very minute insect. I say, without eating its way, because much depends upon whether insects can reach the interior of the receptacles and at the same time carry pollen with them; and writers on the subject, so far as I am aware, have not considered the probabilities of the earlier visiting-insects thus opening a channel for those following. I have also, in another place, suggested the possibility of the scales at the mouth of the receptacle being loosened at the receptive period, and Mr. C. B. Clarke tells me that he has actually observed this to be the case.

The flowers are of four kinds—namely, male, female, neuter, and gall; and they are variously associated, or separated, in different species of *Ficus*. There are indeed five kinds of flowers if we include the pseudo-hermaphrodite flowers of the group *Palæomorphe*. In the cultivated fig (*Ficus Carica*), the flowers are almost invariably all female; and the male flowers of this species are borne by the "caprifig" of the south of Europe and Western Asia. Associated in the same receptacles with the male flowers, and covering the whole of the inside except a ring near the top, are the so-called gall-flowers. Structurally they are female, but instead of bearing seed they nourish the larva of an insect, and the perfect females of this insect are supposed to convey the pollen of the male flowers to the receptacles containing female flowers, the ovules of which are thereby fertilized. The presence of insects in figs seems to be general in the very numerous (500 perhaps) species spread all over the tropical regions of the earth; and the commonly accepted theory is that these insects, in return for the shelter and nourishment received, convey the pollen from the male to the female flowers, so that the association is mutually beneficial. At least this was the theory of Solms and Fritz Mueller. In the introduction to his monograph of the Asiatic figs, Dr. G. King says:—"The exact way in which the females are pollenized is a matter on which I cannot pretend to throw any light. I can only state the problem." Yet a little farther on he states that there can be no doubt that the insect developed in the gall-flowers in some way conveys the pollen of the males to the females in other receptacles, though he found it difficult to understand how this could be effected; and he informed the writer that he had never discovered the slightest evidence of the process, beyond the fact that seeds were formed.

At the instigation of Dr. King, Dr. Cunningham has thoroughly investigated the phenomena of fertilization in *Ficus Roxburghii*, and he arrives at the conclusion that pollen is never, or exceedingly rarely, conveyed to the female flowers, though good seed is abundantly matured.

Ficus Roxburghii is perfectly diœcious—that is, the two sexes are produced on different trees; and the fruit is borne in large clusters on the thicker branches and trunk often at the very base of the same, and extended on the ground.[1] The receptacles are similar in shape to those of the common fig, and from two to three inches in diameter, or sometimes nearly four inches; and the flowers are proportionately large, so that they are easily examined. It may be mentioned, too, that this species belongs to King's section or sub-genus *Neomorphe*, which, in our opinion, exhibits the latest stage in the evolution of the genus.

It would occupy too much space to follow Dr. Cunningham through his investigations, but it will suffice to give some extracts from his concluding remarks:—

"There can be little room for doubt that the phenomena indicate that, while the development of embryos in the female receptacles of the tree is essentially connected with the access of the insects to the receptacular cavity, it is yet normally independent of the introduction of pollen by their agency. The fact that the access of a single insect or of a pair of them only is sufficient to determine the development of ten or twelve thousand embryos, is in itself almost conclusive against the occurrence of any ordinary process of pollination. The obstacles through which a passage has to be forced ere the receptacular cavity is reached are of such nature and amount as to render it almost inconceivable that pollen should be introduced in sufficient quantity, and there is at the same time an absolute want of evidence to show that such introduction takes place. I have carefully examined very many receptacles at various periods shortly after access

[1] A photograph of a tree in fruit forms the frontispiece to the first volume of the "Annals of the Calcutta Garden."

of insects to the cavities, and have never been able to detect any evidence of general distribution of pollen over the stigmatic surface. Examination of individual flowers has given like results; in most cases it has been impossible to find any pollen within the receptacle or cavity, and in the few cases in which any was found it was represented by one or two shrivelled grains adherent to the corpses of insects. It must be borne in mind, too, that, if we accept the hypothesis that the development of the embryos is due to ordinary processes of pollination, we must assume not only that a single insect can convey many thousands of pollen-grains with it in spite of the excessive obstructions to access presented by the ostiolar plug, but that these grains are also methodically and economically distributed, for, unless each stigma were only allowed to appropriate a single grain, the amount introduced would have to be indefinitely multiplied.

"The most important evidence against the occurrence of pollination of any sort as a normal and essential event lies, however, in the fact that the embryo originates, as it does in undoubted cases of development, apart from pollination. The embryo, as a rule—for of course it is possible that pollination and normal evolution may occur in certain individual flowers—certainly arises as an outgrowth of the nucellar parenchyma, outside the embryo-sac, and not as the result of special evolution of any elements contained with in the latter. The embryo-sac up to the period of insect-access and of initial development of the embryo normally retains the characters of a simple uninucleate cell. There is no evidence of the formation of an oosphere, of synergidæ, or of antipodal cells within it, and it is only subsequent to commencing evolution of the embryo that the primary nucleus is replaced by a large number of secondary ones, which are apparently related to the elaboration of food material for the growing embryo, when it gains access to the cavity of the sac.

"But if this be so, if pollination be unnecessary, why should the access of insects be essential to the development of embryos? The phenomena presenting themselves in connection with the male flowers of gall-receptacles appear to afford a clue to answering this question. It is just as impossible for the male flowers to come to perfection—just as impossible for perfect pollen-grains to be developed without the access of insects to the gall-receptacles—as it is for embryos to be developed in female ones under parallel circumstances.

"The development of embryos in *F. Roxburghii*, then, appears normally to be an asexual process dependent on hypertrophic budding of a specialized portion of the nucellar parenchyma, and it appears not improbable that the phenomenon is not peculiar to the species, but is the rule in the case of other figs also. This, of course, requires further investigation; but in the only instance in which I have yet had time to examine the matter—in the case of *F. hispida*—there can be no doubt that it is so."

From the foregoing extracts it will be seen that Dr. Cunningham insists on two extraordinary phenomena—namely, the impossibility of the formation of pollen in the absence of insects, and the formation of embryos by budding outside of the embryo-sac instead of sexual development. As to the first, improbable as it may seem, I am assured by two or three independent observers, who have had opportunities of testing Dr. Cunningham's work, that they have arrived at the same conclusions. As to the second, the asexual formation of embryos is not so very rare an occurrence, according to Strasburger in his elucidation of polyembryony and the so-called parthenogenesis. Then as to the whole, the phenomena would seem to point to the extinction of sexuality. The points are that the development of both pollen and embryo is due to a stimulation of the tissues caused by the punctures of insects. Therefore Dr. Cunningham might with more propriety have entitled his paper "The Phenomena of Non-Fertilization." W. BOTTING HEMSLEY.

SYNONYMY OF THE POLYZOA.

A Synonymic Catalogue of the Recent Marine Bryozoa. By E. C. Jelly, F.R.M.S. (London: Dulau and Co., 1889.

THIS is a work for which all students of the Polyzoa (Bryozoa) should be grateful. It supplies an undoubted want, and will greatly facilitate the investigation of the large and interesting class with which it deals.

Synonymy is certainly not an attractive element of natural history study. Indeed, anything of less intrinsic interest cannot well be imagined, and yet it has a specific value in relation both to morphological and systematic work, and it is of the first importance that it should be carefully determined. A just and accurate synonymy is of course an essential condition of a sound nomenclature; it is a key to the actual state of knowledge, and an index to the sources in which it must be sought, which is invaluable to the student. It is also a safeguard against duplicate and delusive names.

Miss Jelly's "Synonymic Catalogue of the Recent Marine Bryozoa" fills a vacant place. There is not, so far as we know, any work which occupies the same ground. "It aims," as the author explains in her preface, "at bringing into view all the names of *published* recent Bryozoa, with as full a synonymy as may be possible."

The fossil forms belonging to recent species, and these only, are included in the synonymy. The work, which bears the marks of careful and conscientious labour, brings before the student within small compass the entire series of published Polyzoan forms belonging to the recent fauna; supplies him with a reference to the book in which each species was first described and with the name of the writer who first described it; tracks it, as it were, however disguised by variety of name, from author to author, and so in fact furnishes an index to the whole range of the systematic literature.

The value of such a guide to the student of the Polyzoa must be at once evident. It economizes his labour; it enables him to enter upon original investigation with a full knowledge of what has been already done, and it saves him from adding to the weariness and perplexity of those who may follow him by multiplying duplicate names.

Within the last few years large additions have been made to the known species of Polyzoa, the diagnosis of which is distributed to a great extent through the biological journals of Europe and America. With all the care that he can exercise the student is continually liable to overlook some paper in some obscure periodical of which perhaps he has never heard, and, as a result, to add another name to the already burthensome synonymy. In point of fact, as a matter of convenience the synonymy of each class of any extent is worthy of separate and special treatment; and such a work as Miss Jelly has now supplied should be regarded as an essential part of the apparatus of the student who devotes himself to descriptive and systematic work.

We cannot but regret that Miss Jelly has abandoned

the name of the class which has been generally adopted in England, and which commemorates the remarkable researches of an unobtrusive but most able and original worker in the province of invertebrate zoology. We have no intention of reviving the controversy on this subject, in which indeed Miss Jelly takes no part, as she does not state the grounds on which she bases her decision. The *consensus* of Continental naturalists in favour of Ehrenberg's name has no doubt had much weight with her, but the appeal to numbers is hardly likely to satisfy those who have tried the case on its merits, and arrived at a different conclusion. Of course it would be satisfactory to secure uniformity of nomenclature, if it were possible; but those who have a strong conviction that J. V. Thompson was the earliest to appreciate and define the distinctive peculiarities of Polyzoan structure, and that his name was intended to apply not merely to the zooids of a colony, but to the type of organization which they represent, can hardly consent to be parties to an absolute rejection of his claim.

The value of such works as the present depends entirely on the care and minute accuracy with which they are compiled. Miss Jelly's "Catalogue" affords abundant proof that these qualities have not been wanting in her case. That she possesses a thorough command of the literature of her subject is shown by the fulness of the synonymy, and (very strikingly) by the explanatory notes appended to many of the species. The book supplies ample evidence of intelligent and enthusiastic interest in the subject, and patient industry in dealing with it.

As to the synonymy itself, many difficult questions arise in connection with it, which clearly cannot be discussed in a work which aims at being a guide to the recorded species, and not a critical treatise. In not a few cases of supposed synonymy we should feel compelled to dissent from the conclusions arrived at, or adopted from others. But on such points the student must satisfy himself.

We may add that the book is handsomely got up, and printed in a type which, so far as clearness is concerned, leaves nothing to be desired.

Miss Jelly is to be congratulated on the completion of a very onerous task, and on the valuable contribution which she has made to the working apparatus of the student.

OUR BOOK SHELF.

Zoologische Ergebnisse einer Reise in Niederlandsch Ost-Indien. Von Dr. Max Weber. Erstes Heft. (Leiden: Brill, 1890.)

THE numerous books, memoirs, and pamphlets that have been published during the century dealing with the natural history of the Malay Archipelago have revealed to the world of science a fauna which is perhaps unrivalled for richness, variety, and general interest. But even now we are scarcely beyond the threshold of the investigation. The travels of von Rosenberg, Wallace, van Martens, Forbes, and others, and the painstaking observations and collections of many of the Dutch residents and *controleurs*, have given us a knowledge of the principal features of the ornithology and entomology of some of the more important islands, but there are still many regions of undoubted interest that have scarcely been explored at all, even by their nominal masters the Dutch. During the journey undertaken by Prof. Weber and his wife in 1888 and 1889 some of these little-known regions and islands were visited, and the results are now appearing in a series of memoirs prepared by eminent Dutch naturalists, under the editorship of the distinguished Professor of Zoology at Amsterdam.

Several interesting new forms are described in the first part; and no doubt many others will follow in the memoirs that are now in course of preparation, from Central Celebes, Flores, and the Saleyer Islands—regions that have hitherto scarcely been visited by naturalists. But the interest in Prof. Weber's results does not by any means lie exclusively in the fauna of the remote corners of the archipelago. By investigating the fresh-water fauna and the inconspicuous forms of terrestrial invertebrates of all the districts visited, he has opened to us a new chapter in the natural history of the archipelago. The memoirs on Spongillidæ by the editor, on Apterygota by Oudemans, and on the land Planaria by Loman, are most valuable and interesting contributions to our knowledge of the tropical species of these groups. Of more than special importance is the paper by the Professor and Madame Weber, "On some New Cases of Symbiosis." One of the most remarkable of those described is that of an alga belonging to the family Trentepohliaceæ symbiotic with a fresh-water sponge. This paper is in French, and contains an interesting discussion of the problems presented and the literature of the subject.

Two memoirs, in English, by Prof. Weber and by Dr. Jentinck, deal with the mammalia collected during the journey. It appears from these pages that the curious animal the sapiutan, *Anoa depressicornis*, is not confined to the northern peninsula of Celebes, as is usually stated to be the case, but may be found in favourable localities all over the island. In a long discussion on the habitat of the rare monkey *Macacus maurus*, we are told that it occurs on the Maros River and elsewhere in South Celebes, and that it should be considered to be "one more of the remarkable animals peculiar only to that island, with a continental character."

Every naturalist who reads this first part of Prof. Weber's results will look forward with interest to the publication of the memoirs that are still in course of preparation.

SYDNEY J. HICKSON.

Inorganic Chemistry, Theoretical and Practical. By William Jago, F.C.S., F.I.C. (London: Longmans, Green, and Co., 1890.)

THE number of classes established according to the regulations of the Science and Art Department is now so considerable that publishers and authors alike are ready to specially cater for their needs. Messrs. Longmans, Green, and Co. have already an extensive series of "science manuals" written to meet the requirements of students taking the elementary stage of subjects as given in the Directory of the Department; and Mr. Jago's volume is one of a similar series that is in preparation to include the matter prescribed in the advanced syllabus of each subject. After the preface we are nowhere reminded of the particular aim of the book, or of the limitations under which the author has done his work; and it is worthy of note that the papers set by the Department examiners are not reprinted at the end. Leaving the particular meaning attached to the words elementary and advanced by the Science and Art Department, we may describe the volume before us as an elementary treatise on inorganic chemistry, of about 460 pages. It includes a consideration of the more common metals and their compounds, three pages concerning the periodic law, and six pages on the "causes which modify chemical action." Fluorine takes its natural place among the halogens as an isolated element, and other of the recent advances are duly noticed. Some parts are very meagre, as, for example, the paragraphs

on acetylene, in which, by the way, there occurs the very objectionable expression "two volumes of carbon" in reference to the composition of the gas. The statement that "nitric acid is largely used in the manufacture of sulphuric acid" needs qualification, especially as we are told that "sulphuric acid is largely employed in the preparation of . . . nitric acid." But, on the whole, the manual is one that deserves recommendation, and will be valued by those for whose use it has been written.

We would suggest that, in the future editions that will doubtless be called for, "choke" or "after-damp" be not described as carbon dioxide, because the amount of carbon dioxide in it is very small compared with the nitrogen present; and we do not think that the examiners at South Kensington would harshly judge any student who corrected the current notion. And if the engraver of the illustrations had photographs of the apparatus supplied him, or, better still, if the blocks could be prepared mechanically from such photographs, the figures would have an appearance of genuineness which at present many of them lack. It is better to represent the apparatus used than the operation in progress, and then one avoids such unnecessary and unwise conventionalities as appear in the attempt to illustrate a brilliant combustion in a glass jar.

Arithmetical Chemistry. Part I. By C. J. Woodward, B.Sc. New Edition. (London: Simpkin, Marshall, and Co., 1890.)

IN the study of chemistry there is a certain amount of arithmetical work which the elementary student must master as he progresses, and this used to be generally considered as the part in which the young pupil was most likely to fail. But now it is different, and we fear that there is rather a danger of too much stress being laid upon arithmetical exercises. In the volume before us the author proceeds by easy stages, explaining the various subjects dealt with in a sound and simple manner. We hope for the student's sake that it is intended for the teacher to select from the numerous exercises set. At the end of the volume there are "the whole of the questions in arithmetical chemistry and chemical philosophy," selected from the examination papers of five different examining bodies, for the years 1886 to 1889 inclusive. These will doubtless be useful to the teacher if used with discretion enough to prevent his students from imagining that chemistry is a branch of arithmetic.

Air-Analysis: with an Appendix on Illuminating Gas. By J. Alfred Wanklyn and W. J. Cooper. (London: Kegan Paul, Trench, Trübner, and Co., Ltd., 1890.)

THIS small volume of ninety pages is a practical guide to air-analysis, especially for sanitary purposes. The directions are plain, and multiplication of methods is avoided. Hempel's apparatus is employed. For the estimation of oxygen, nitric oxide is advised, and it is pointed out that as an excess of the gas is used it need not be pure. The authors state that, in their hands, this method has proved very accurate, and they give experimental results showing that it is to be preferred to alkaline pyrogallol or explosion with hydrogen. Directions for these latter methods are, however, included. The estimation of small quantities of carbonic oxide is performed by absorption in a cuprous chloride solution, with subsequent elimination and measurement of the gas. The analysis of coal-gas is dealt with in the appendix, and the volume concludes with some useful tables. As an addition to the treatises on special branches of analysis written by Mr. Wanklyn, either solely or jointly, this volume will be welcomed by analysts and students.

Fresh-water Aquaria: their Construction, Arrangement, and Management. By Rev. Gregory C. Bateman. (London: L. Upcott Gill, 1890.)

MR. BATEMAN says that he has always been fond of natural history, and that when he was a boy he looked forward with pleasure to the prospect of having an aquarium of his own. When this delight was experienced, he found that it had many drawbacks. These were due to the fact that he did not know how to manage his treasure. He bought or borrowed books on the subject, but was not able to obtain all the information he required. Then he tried to find out by experiment what he could not learn by reading; and as most of his attempts were in the end successful, he resolved that he would write such a book as he himself had wished for when he was making his "first blunders in aquarium matters." The present volume is the result of this decision, and there can be no doubt that it will be very cordially welcomed by many students who want just such information as the author has brought together. He writes simply, clearly, and practically, and no one who reads with moderate care what he has to say will find much difficulty in complying with the rules he lays down. He gives, also, interesting details as to the best water-plants and live stock to be kept, how and where they are to be obtained, and how they are to be maintained in health. The volume includes many illustrations.

Scenes and Stories of the North of Scotland. By John Sinclair. (Edinburgh: James Thin. London: Simpkin, Marshall, and Co. 1890.)

MR. SINCLAIR is an intelligent and lively writer, and has produced a book which may be read with pleasure by persons who have visited, or think of visiting, the scenes he describes. The work is not, in the strict sense, scientific; but it includes many passages which are, to a certain extent, of scientific interest. The subjects are: Loch Duich, Ross-shire; the Black Rock, Ross-shire; the Island of Lewis; Assynt, in Sutherland; the Caithness coast; the town of Thurso; and the Shetland Islands. Here is ample scope for fresh observation and bright description; and the author has generally made good use of his opportunities. It is to be regretted, however, that he did not, before writing of the Island of Lewis, make himself acquainted with what trustworthy archæologists have said about the great prehistoric monument at Callernish. "There is little doubt," he asserts, "that these standing stones are a monument of the ancient Druids." There is not a shred of evidence that the Druids had anything whatever to do either with these or with any other "standing stones."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Discharge of Electricity through Gases.

ON returning from abroad I find that Prof. J. J. Thomson has written to you to complain about a passage in my Bakerian Lecture, and I should like to say a few words in explanation. I am sorry if I have said anything that would seem unfair to Prof. Thomson, but I have re-read his paper, and confess that my difficulties have not been cleared away.

I shall be glad to be allowed to enter somewhat fully into the objection raised by Prof. Thomson to my remarks, as, independently of any personal question, this may help to clear up some disputed matters. The point at issue between Prof. Thomson and myself is, whether the Clausius-Williamson dissociation hypothesis forms an essential portion of the views on

the disruptive discharge which he expressed in his paper published in the *Philosophical Magazine* for June 1883. That is partly a matter of opinion, but Prof. Thomson has certainly led his readers to think that he considered the presence of free atoms before discharge as essential.

One of the principal difficulties of any theory of the disruptive discharge is the explanation of the relation between pressure and so called dielectric strength. This relation is one of the crucial points by means of which every theory must be judged. The two following passages will show how Prof. Thomson meets the difficulty :—

"Let us now apply these considerations to the case of the electric discharge. The disturbance to which the gas in an electric field is subjected makes the molecules break up sooner into atoms than they otherwise would do, and thus diminishes the ratio of the paired to the free times of the atoms of the gas; as the intensity of the electric field increases, the disturbance in some places may become so violent that in these regions the ratio of the paired to the free times approaches the value it has when the gas is about to be dissociated."

"Let us now consider what effect rarefying the gas would have upon its electrical strength. In a rare gas the mean distance between the molecules is greater than in a dense one; and if the temperature be the same in both cases, and consequently the mean velocity of the molecules the same, the ratio of the free to the paired time will be greater for the rare than for the dense gas, for the free atoms will, on an average, be longer in meeting with fresh partners. Thus the rare gas will be nearer the state in which it begins to suffer dissociation than the dense gas, and thus it will not require to be disturbed so violently as the dense gas in order to increase the ratio of the free to the paired time to its dissociation value; and thus the intensity of the field necessary to produce discharge would be less for the rare gas than for the denser one: in other words, the electric strength would diminish with the density, and this we know is the case."

It will be seen that the explanation entirely depends on the idea that free atoms exist already before discharge. In my Bakerian Lecture, I have pointed out that the existence of free ions seems inconsistent with experimental fact, and I add: "This seems to me to be fatal to J. J. Thomson's view of the disruptive discharge." This is the passage which Prof. Thomson says implies a misconception, but surely, assuming that I am right in my argument that no dissociation takes place before discharge, I am also right in saying that this is fatal to any theory which makes the relation between pressure and spark potential depend on such dissociation. It may be said, on the other side, that the idea of decomposition of molecules by the discharge, which forms so important a feature in the theory which I have explained in my Bakerian Lecture of 1884, occurs already in Prof. J. J. Thomson's paper of 1883; and that it is unfair, therefore, to condemn his views because they do not account for a feature of the discharge which has never been satisfactorily explained. A few remarks are therefore necessary to explain the relationship between Prof. Thomson's paper of 1883 and my own of the succeeding year.

The hypothesis that the discharge of electricity in gases is similar to that in electrolytes, and that each atom of a gas such as nitrogen or hydrogen carries a permanent charge, seems so obvious that it must have occurred to many who have thought about the matter; but no attempt has, until recently, been made to develop the hypothesis so as to account for the complicated phenomena of the discharge.

The credit of being the first to have done so undoubtedly belongs to Giese, who has explained by means of it a number of observed facts concerning the behaviour of flames, and of the gases rising from flames, but he has not until quite lately considered any other part of the subject. J. J. Thomson, in 1883, published his paper "On the Theory of the Electric Discharge in Gases." The hypothesis of atomic charges is not mentioned therein, and the author does not discuss the question whether or not a current of electricity in a gas consists of a diffusion of charged atoms or not. So carefully are these matters excluded, that it is difficult to avoid the conclusion that it was done intentionally, in order that the investigation may be more general, and independent of any particular theory which might in future be established.

"As the most general assumption," the electric field is supposed to consist of "a distribution of velocity in the medium whose vortex motion constitutes the atoms of the gas; the disturbance due to this distribution of velocity will cause the molecules to break up sooner than they otherwise would do." The way in which the decomposition of molecules is connected with the spark must be judged from the passages quoted above, and from those quoted by Prof. Thomson in his letter to you. The general terms in which the whole paper is expressed may increase its scientific value, but it affords no help to those who wish to form any more definite notion what an electric current through a gas consists of. If in future it should be shown that a current of electricity does *not* consist of a diffusion of charged atoms, Prof. Thomson's reasoning may still apply. I do not know whether such general considerations may be fitly described as "a theory."

After having for a number of years attempted to trace out the consequences of the electrolytic hypothesis, and discovered some method by means of which it could be definitely tested, I presented to the Royal Society, in 1884, a paper which forms the subject of the Bakerian Lecture for that year. In that paper I referred to Prof. J. J. Thomson's of the previous year, but unfortunately I was not then aware of Giese's work, and that gentleman has undoubtedly some right to complain of the way in which his researches have, till recently, been neglected by myself and others. I have never claimed that my hypothesis of atomic charges was in any way original; but I have always maintained that that hypothesis, by itself alone, does not explain much. My recent Bakerian Lecture shows sufficiently clearly that we must form much more definite notions regarding the phenomena of dissociation and the interaction between chemical and electrical forces before we can say that we have a complete theory of the electric discharge, and those who will overcome successfully all the remaining difficulties will have done much more than those who started the idea. The work of Hittorf, E. Wiedemann, Hallwachs, Warburg, Elster and Geitel, and others, has thrown so much light on many points, that the final decision as to the truth or otherwise of the theory under discussion cannot be long deferred. I have ventured to call it the "theory of electrolytic convection," which, I should say, was a sufficiently neutral and distinctive name.

ARTHUR SCHUSTER.

A Suggestion respecting the Syllabus of the Science and Art Department.

WILL you permit me to call attention to the following considerations which have occurred to myself and several of my colleagues as the result of some years' teaching experience?

(1) The syllabus of Subject VIII. (Sound, Light, and Heat), as it at present stands, is very extensive—too much so, indeed, for the majority of students to grasp in one session. This fact is tacitly acknowledged by the Department, as a student is permitted to obtain a first class in either stage by taking two only out of the three subjects.

(2) Sound and light are pretty closely related, both being forms of wave motion, and the general ideas involved in their study very similar. But between these two subjects on the one hand, and heat on the other, this connection is small, existing, indeed (so far as the Department's syllabus extends), only in the comparatively unimportant section of radiant heat. The considerations involved in dealing with specific and latent heat and with heat as a form of energy are of an utterly different character from those presented in sound or light.

(3) The syllabus of Subject VI. (Theoretical Mechanics) is also too extensive for most students to grasp in one session, including, as it does, four subjects, viz. Statics, Dynamics, Hydrostatics, and Pneumatics. And although the Department does not officially state that a student can obtain a first class with two only of these subjects, yet the papers are always arranged to admit of his doing so by taking statics and dynamics only. Thus, in the elementary stage, a candidate may answer only seven questions out of twelve, and in the advanced, eight out of twelve, and yet in each case *nine* of the twelve are confined to statics and dynamics.

The result is that teachers and students pay but scant attention to hydrostatics and pneumatics.

(4) A large number of students take physics and mechanics simply as accessories to engineering and the applied sciences. Now, to such students a knowledge of *heat* is most essential, while sound and light are quite useless. Again, though heat has but slight connection with sound or light, it has a very strong connection with hydrostatics and pneumatics. By far the most important thermal phenomena are those presented by liquids and gases, and moreover it is precisely these that an engineering

student requires to know. For how can the action of the steam-engine be properly understood without a knowledge of the principles of fluid and gaseous pressure, and of the relation between heat and work? Yet under the present arrangement the former of these constitutes a neglected part of Subject VI., while the latter comes under Subject VIII., and is associated with matter totally irrelevant.

All these considerations seem to point to the desirability of a change in the official syllabus somewhat as follows:—

(*a*) To cut out Heat from Subject VIII., making the latter consist of Sound and Light only.

(*b*) To cut out Hydrostatics and Pneumatics from Subject VI., making the latter consist of Statics and Dynamics only.

(*c*) To combine Heat, Hydrostatics, and Pneumatics into a new subject having its appropriate number. These three could then be more effectively studied than under the present system, and there would be ample matter therein to form one of the courses from September to May. The syllabus of the new subject would naturally include all the points specified by the Department as necessary preliminaries to the study of Steam (*vide* Steam syllabus, Subject XXII.), and would thus supply a specific want to all engineering students.

On the whole, it is respectfully submitted to the authorities of the Department, and others interested in the education of the people, that the proposed alteration would conduce to a more thorough and systematic study of all the subjects referred to, and be attended with benefit to students both of physics and mechanics. VOLO LEGES MUTARI.

On Last-place Errors in Vlacq.

M. M. F. LEFORT, in his account of the great Cadastre tables, contained in the fourth volume of the *Annales de l'Observatoire Impérial de Paris*, gives a list of errors in Adrian Vlacq's ten-place table of logarithms. As this one by Vlacq, or its copy by Georg Vega, is the only complete table of ten-place logarithms yet in existence, we naturally desire to make it thoroughly accurate, and therefore proceed to correct it by aid of this new information.

M. Lefort tells us that Prony, in his instructions, was expressly enjoined "not only to compute tables which shall leave nothing to be desired as to exactitude, but to make them the most vast and imposing monument of calculation that had ever been made or even conceived," and, adds M. Lefort, "this programme, so widely sketched, has been faithfully carried out." Yet, on the very same page, we are told "that Prony fixed the general limit of precision for his logarithmic tables at 12 decimals"; this although the original work by Henry Briggs had been carried to 14 places.

Thus it seems that the Cadastre tables cannot be trusted to determine the absolute accuracy of those of Vlacq whenever the figures to be rejected are between the limits 4900 and 5100, and that in no case can they serve to check the final figures in Briggs.

Having scrupulously examined, by help of my fifteen-place table, all the corrections given by M. Lefort, I here give the results, in order that the possessors of Briggs, Vlacq, or Vega may make note of them.

Among 282 last-place corrections given, I find seven to be erroneous, the logarithms in Vlacq and in Vega being right. In order to make doubly sure, I have also used my 28-place table, and here give the exact figures from the 8th to the 20th place—

Number.		Logarithm.
26188	...	322 49959 00920
29163	...	978 49968 31667
30499	...	999 50010 73882
31735	...	026 49975 27403
34182	...	883 50038 92375
34358	...	753 50011 99957
60096	...	662 49998 09339

From this we see that the Cadastre tables are inadequate to the thorough checking of ten-place logarithms; in the case of the last of these miscorrections, even the fifteen-place table is barely sufficient, and needs to be fortified by an extended calculation.

Among the 275 remaining errors, five have been imported from Briggs, and I have therefore examined them to greater length; the logarithms to the 20th place are—

Number.		Logarithm.
7559	...	453 41468 90981
8006	...	857 69086 31797
8009	...	936 63054 38960
10033	...	122 46398 29224
99926	...	031 14867 68936

Thus there are left 270 errors to be charged against Vlacq; of these no less than 96 are within the limits of inaccuracy allowed by Prony.

Near the end of the list there occurs a group of 21 (from the number 98336 to 98367) which seem to have resulted from some single running error. Now this part of the table was copied from Briggs, and we should expect these errors there; but, on turning to the original work, we find that none of his logarithms differs by more than unit in the 14th place from that of the fifteen-place table, and thus the source of the errors in Vlacq becomes mysterious.

The most feasible explanation is that the errors had been observed and corrected while the sheet was at press, and that thus all the copies of Briggs are not alike. It is probable that the very copy used by Vlacq may be preserved in some one of the libraries in the Netherlands; in such case, an inspection would set the matter at rest. EDWARD SANG.

September 27.

On the Soaring of Birds.

IN answer to Mr. C. O. Bartrum's objections in NATURE of September 4 (p. 457), I beg to refer to an article in the *Skand. Archiv. für Physiologie*, ii. 2, in which I have given a detailed account of the weighty reasons which have led me to suppose that soaring birds are able to undertake successive alterations of direction with very little loss of *vis viva*. This loss is of the same kind as that caused by the resistance of the air to the rectilinear translation.

There is, however, one fact which, in the article in the *Skand. Archiv.*, I have thought it superfluous to point out—namely, that the manœuvre of the bird is the same, and the loss of energy thereby equally the same, whether the bird turns in a calm or in a uniform wind. If Mr. Bartrum has been led to another opinion, it may be that he has not quite made out how these turnings are executed. MAGNUS BLIX.

Lund, Sverige, October 10.

Earthquake Tremors.

IF those of your readers who are interested in this subject will turn to p. 84 of the "Report on the East Anglian Earthquake of April 2, 1884," by R. Meldola and W. White (Essex Field Club special memoirs, vol. i.), they will see that at Wivenhoe a man who felt the shock of the earth movement found to his own satisfaction, by careful measurement and calculation, that the vertical displacement where he stood amounted to no less than *six feet*. How it was that any building in Wivenhoe remained standing after so tremendous an upheaval the observer did not appear to think worth considering.

ALFRED P. WIRE.

THE PROPERTIES OF LIQUID CHLORINE.

ALTHOUGH chlorine was shown by Faraday so long ago as the year 1823 to be one of the more easily condensable gases, yet, no doubt owing in a large measure to its very disagreeable nature, comparatively little has hitherto been known concerning its properties when in a liquefied state. In view of the fact that chlorine is now stored in the liquid state for the use of manufacturing chemists in a similar manner to carbon dioxide, sulphur dioxide, and ammonia, it is imperative that something more definite should be known as to the relations of liquefied chlorine to temperature and pressure. Consequently, a very complete investigation of the subject has been made by Dr. Knietsch at the request of the directors of the "Badischen Anilin- und Sodafabrik," of Ludwigshafen; and his results, of which the following is a brief account, are published in an interesting communi-

cation to the current number of *Liebig's Annalen* (Band 259, Heft 1, p. 100).

The work includes the determination of the vapour-tension of liquid chlorine at temperatures from -88° C. to 146° C. (its critical point), a complete examination of its behaviour near the critical point and the determination of its specific gravity and coefficient of expansion for a range of temperature between -80° and $+80^\circ$.

Liquid chlorine generally appears to possess a yellow colour. When, however, the colour of a long column is examined, it is found to have a distinctly orange tint. The absorption spectrum does not exhibit any characteristic bands, but the blue and violet portions of the spectrum are completely absorbed, the transmitted spectrum thus consisting of the red, orange, yellow, and green.

Vapour-Tension of Liquid Chlorine below its Boiling-Point.

The apparatus used for this determination consisted of a kind of distilling flask, whose side tube was connected by means of a piece of strong-walled caoutchouc tubing with a wide manometer tube. The flask was about half filled with liquid chlorine, and was immersed in a bath also containing liquid chlorine whose temperature could at the same time be kept equal throughout, and be very finely regulated by means of a current of air driven in through a tube passing to near the bottom of the bath.

In commencing a series of determinations the chlorine in the flask was first made to boil, thereby driving out the air remaining in the apparatus. The neck was then closed by means of a caoutchouc stopper well coated with glycerine, and the open end of the manometer was allowed to dip into a vessel containing concentrated sulphuric acid. As the flask became cooled by immersing it in the cold chlorine in the bath, sulphuric acid was drawn into the manometer until it attained a height of 3–5 cm., when the caoutchouc connection was momentarily pinched while the open end of the manometer was transferred to the mercury trough. The small column of sulphuric acid thus standing above the mercury column effectually protected it from the corroding action of the chlorine. The bath was then cooled gradually, and a series of readings taken of the temperature of the bath, by means of an alcohol thermometer, and of the position of the meniscus of the mercury in the manometer. The small column of sulphuric acid was of course calculated to its equivalent height of mercury, and added to the measured height of the mercurial column. By careful use of the current of dry air the liquid chlorine of the bath was found capable of being reduced in temperature as low as -60° C. The lower temperatures, down to -88°, were attained by mixing more or less solid carbon dioxide with the chlorine. The results obtained are given in the table at the end.

Determination of the Pressure of Liquid Chlorine from its Boiling-Point to 40° C.

The data at present existing upon this subject are very meagre and conflicting. Davy and Faraday found the pressure at 15° C. to be 4 atmospheres, whilst Niemann gives the pressure at 0° C. as 6 atmospheres, and at $12^\circ\!\cdot\!5$ C. as 8 atmospheres. As this is a most important point in regard to the storage of liquid chlorine in metallic bottles, great pains have been taken to arrive at unimpeachable results, and as the most certain method of measuring the pressure a high column of quicksilver was employed. The apparatus consisted of a U-tube, one limb of which was narrower than the other, and prolonged upwards to a height of over 8 metres. The other and wider limb was joined at the top by means of a capillary tube to a cup, serving the purpose of a funnel for introducing the liquid chlorine. In commencing an experiment, a convenient quantity of mercury was first poured in so as to stand in the wider limb at about a quarter its height. A column of sulphuric acid was then introduced into the wider limb so as to protect the mercury, and finally the liquid chlorine was introduced through the funnel by a process of alternately warming and cooling; the cooling was effected by pouring a little liquid chlorine over a piece of cotton wrapped round the limb and evaporating it by a strong current of air. When the limb was quite full, the chlorine occupying the capillary tube was evaporated by the warmth of a small blowpipe flame, and the capillary fused up. The apparatus was then immersed, until the wider limb was covered, in a bath of liquid sulphur dioxide for temperatures up to 0°, in ice for the determination at 0°, and in water agitated by a current of air, and either cooled by ice or warmed by a small flame for temperatures up to 40°. For the comparatively higher of these temperatures it was of course necessary to pour mercury into the longer limb so as to prevent the mercury in the wider limb being driven round the bend. Complete results are given at the end, but it may be remarked in passing that the pressure at 0° is 3·66 atmospheres; and at 15°, 5·75 atmospheres.

Determination of the Pressure at Higher Temperatures.

For these yet more dangerous and difficult experiments a metal apparatus was employed, similar in principle to that just described, except that the pressures were measured by a metal gauge manometer, which had previously been completely tested and its readings verified. It was found important in these experiments not to employ too much chlorine, as owing to the immense coefficient of expansion the whole space might become full of liquid, and further heating would cause the generation of dangerously high pressures. For temperatures up to 100° a water-bath was employed, and for the higher temperatures up to the critical point 146° an oil-bath, both kept in circulation by a rapid current of air. The pressure at the critical temperature of 146° C. was found to be as high as 93·5 atmospheres.

Critical Point of Liquid Chlorine.

The critical point was determined in a separate experiment, and some very interesting results were obtained, the yellowish green colour of chlorine perhaps assisting in rendering the appearance of what has sometimes been termed the fourth state of matter between the liquid and the gaseous more distinct than usual. A hard glass tube of 8 mm. diameter was about one-third filled with redistilled dry liquid chlorine and sealed. A small thermometer, whose readings commenced at 140°, was attached to it by platinum wire, and the whole very slowly heated in a bath of vaseline. The observations were made with the naked eye, the observer being protected from any possible explosion by a thick glass plate. At 140° extremely small bubbles began to be developed throughout the mass of liquid. At 144° the hitherto sharp meniscus began to disappear, and at 145° the presence of a liquid was only evident by the more intense yellow colour and higher refractive power of the lower portion of the tube. At 146° the contents of the tube were homogeneous throughout, the critical point being attained, and the liquid converted into gas. On allowing the tube to cool slowly, the condensation always commenced below 146°, with the formation of a cloud and a fine rain of minute yellow spheres of liquid chlorine. The rain was generally apparent throughout the whole of the upper portion of the tube. Sometimes, however, the liquid meniscus again appeared without any previous manifestation of precipitation.

Specific Gravity and Expansion of Liquid Chlorine.

It is a curious fact that many gases when compressed to the state of liquid expand enormously when heated as compared with ordinary liquids, the amount of expansion sometimes exceeding that of the gas itself. Liquid chlorine is no

exception to this rule, and it was absolutely essential that its rate of expansion should be thoroughly investigated, in order that storage bottles should not be filled to a dangerous extent. For temperatures up to 36° C. a closed dilatometer of glass was employed, the long cylindrical bulb of 60 c.c. capacity and part of the stem being filled with liquid chlorine, and the remainder of the stem with chlorine gas. The whole apparatus was immersed in a long cylindrical bath. For the lowest temperature, of – 80°, the bath was filled with solid carbon dioxide. For the determination of the specific gravity at the boiling-point of chlorine, a bath of boiling liquid chlorine itself was employed, no less than three kilograms being required. Between the boiling-point and 0° the substance used in the bath was liquid sulphur dioxide. For the determination at zero powdered ice was employed, and for the higher temperatures a water-bath kept in motion by an air current. It was not possible to proceed higher than 36° with this apparatus, on account of the danger of explosion. The higher determinations were made by means of a hydrometer suspended in liquid chlorine enclosed in a tube of hard glass which was immersed in a glass water-bath heated to the required temperature.

It will be seen from the following table that liquid chlorine is indeed a very expansible substance. The coefficient of expansion at 80° is already 0·00346, nearly equal to that of gaseous chlorine, and is rapidly increasing, so that before the critical temperature of 146° is attained, the coefficient of expansion will be considerably higher than that of the gas.

Following is a table showing the pressure, specific gravity, and coefficient of expansion of liquid chlorine for every 5° of temperature from – 80° C., calculated from the formulæ derived from the experimental data obtained.

Temperature.	Pressure.	Specific gravity.	Mean coefficient of expansion.
– 102° C.	Solid (Olzewski).	—	
– 88	37·5 mm. Hg.	—	
– 85	45·0 ,,	—	
– 80	62·5 ,,	1·6602	
– 75	88·0 ,,	1·6490	
– 70	118 ,,	1·6382	
– 65	159 ,,	1·6273	
– 60	210 ,,	1·6167	
– 55	275 ,,	1·6055	0·001409
– 50	350 ,,	1·5945	
– 45	445 ,,	1·5830	
– 40	560 ,,	1·5720	
– 35	705 ,,	1·5589	
– 33·6	760 ,,	1·5575	
– 30	1·20 atmospheres	1·5485	
– 25	1·50 ,,	1·5358	
– 20	1·84 ,,	1·5230	
– 15	2·23 ,,	1·5100	0·001793
– 10	2·63 ,,	1·4965	
– 5	3·14 ,,	1·4830	
0	3·66 ,,	1·4690	
+ 5	4·25 ,,	1·4548	0·001978
+ 10	4·95 ,,	1·4405	
+ 15	5·75 ,,	1·4273	0·002030
+ 20	6·62 ,,	1·4118	
+ 25	7·63 ,,	1·3984	0·002190
+ 30	8·75 ,,	1·3815	
+ 35	9·95 ,,	1·3683	0·002260
+ 40	11·50 ,,	1·3510	
+ 50	14·70 ,,	1·3170	0·002690
+ 60	18·60 ,,	1·2830	
+ 70	23·00 ,,	1·2430	0·003460
+ 80	28·40 ,,	1·2000	
+ 90	34·50 ,,		
+ 100	41·70 ,,		
+ 110	50·80 ,,		
+ 120	60·40 ,,		
+ 130	71·60 ,,		
+ 146	93·50 ,,	Critical point.	

An interesting result, which is not noticed by Dr. Knietsch in his paper, is obtained on calculating the specific volume of chlorine from the determination of specific gravity at the boiling-point, — 33°·6. On dividing the atomic weight 35·5 by 1·5575, the specific gravity at the boiling-point, the number 22·8 is obtained for the atomic or specific volume of chlorine, a number practically identical with that derived by calculation from the numerous determinations of the specific volume of compounds containing chlorine.

In this respect chlorine resembles bromine and the compound radicles NO_2 and CN, which were shown by Prof. Thorpe (Journ. Chem. Soc., 1880, 382) to occupy the same volume in the free state as in combination.

A. E. TUTTON.

ELECTRICAL STORMS ON PIKE'S PEAK.

THE "Annals of the Astronomical Observatory of Harvard College," vol. xxii., contains the meteorological observations made at the summit of Pike's Peak, Colorado, at a height of 14,134 feet above sea-level. It is not remarkable that such an elevated station should be celebrated for its electrical storms, and the observers from 1874 to 1888 have recorded many interesting details in the journals respecting their physical and physiological actions.

The manifestation of atmospheric electricity by induced effects is often very strongly marked. During the passage of electrified clouds over the summit of the peak the well-known singing and buzzing noises described as an adjunct of St. Elmo's fire were heard to proceed from the telegraph wires, the exposed instruments, the instrument shelter, and the house. The sound is said to be very similar to the buzzing of bees and crackling of burning evergreens. At times the hair of the observers became upright and strongly repellent, and the same peculiar noise proceeded from it.

Some very remarkable effects are recorded on August 18, 1877:—"During the evening the most curiously beautiful phenomena ever seen by the observer were witnessed, in company with the assistant and four visitors. Mention has been made in journal of May 25 and July 13 of a peculiar 'singing' or rather 'sizzing' noise on the wire, but on these occasions it occurred in the day-time. To-night it was heard again, but the line for an eighth of a mile was distinctly outlined in brilliant light, which was thrown out from the wire in beautiful scintillations. Near us we could observe these little jets of flame very plainly. They were invariably in the shape of a quadrant, and the rays concentrated at the surface of the line in a small mass about the size of a currant, which had a bluish tinge. These little quadrants of light were constantly jumping from one point to another of the line—now pointing in one direction and again in another. There was no heat to the light, and when the wire was touched only the slightest tingling sensation was felt. Not only was the wire outlined in this manner, but every exposed metallic point and surface was similarly tipped or covered. The anemometer cups appeared as four balls of fire revolving slowly round a common centre: the wind-vane was outlined with the same phosphorescent light, and one of the visitors was very much alarmed by sparks which were plainly visible in his hair, though none appeared in the others'. At the time of the phenomenon snow was falling, and it has been previously noticed that the 'singing' noise is never heard except when the atmosphere is very damp, and rain, hail, or snow is falling."

These displays were described with the same minuteness on June 7, 1882. It was then noticed that when the finger was passed along the line the little jets of flame were successively puffed out, to be instantly relighted in the rear. An observer also found that when he approached

one of the places from which the buzzing sound proceeded it would cease, but would recommence again as soon as he withdrew two or three feet distant.

It is recorded that the "observer, on placing his hands close over the revolving cups of the anemometer where the electrical excitement was abundant, did not discover the slightest sensation of heat, but his hands instantly became aflame. On raising them and spreading his fingers, each of them became tipped with one or more cones of light nearly three inches in length. The flames issued from his fingers with a rushing noise, similar to that produced by blowing briskly against the end of the finger when placed lightly against the lips, and accompanied by a crackling sound. There was a feeling as of a current of vapour escaping, with a slight tingling sensation. The wristband of his woollen shirt, as soon as it became dampened, formed a fiery ring around his arm, while his moustache was electrified so as to make a veritable lantern of his face. The phenomenon was preceded by lightning and thunder, and was accompanied by a dense driving snow, and disappeared with the cessation of snow."

A few instances are given of convulsive muscular contraction caused by discharges. Thus, on June 23, 1887, whilst an observer was examining the iron joints around the station, from which the above-described hissing noise was proceeding, a strong electrical manifestation was felt by a twitching of the muscles of the face and hands. A violent "return shock" was experienced by the observer, who, on June 16, 1876, "whilst sitting on a rock, saw a blinding flash of lightning dart from a cloud seemingly not more than five hundred feet away, and heard a quick deafening report, and at the same time received a shock that jerked his extremities together as though by a most violent convulsion, and left lightning sensations in them for a quarter of an hour afterwards."

Among other effects previously noticed at considerable elevations above sea-level we find that on one occasion an observer felt a pain as if from a slight burn on both temples directly under the brass buttons of his cap; when he put his hands to the spots, there was a sharp crack, and all pain disappeared. A peculiar burning sensation was also often felt on the face and hands, and the scalp appeared to be pricked with hundreds of red-hot needles. A more intense action is recorded on June 9, 1882, when an observer was "raised off his feet by the action of the electricity passing through the top of his hat. Instantly snatching the hat from his head, he observed a beam of light as thick as a lead pencil, which seemed to pass through the hat, projecting about an inch on either side and remaining visible for several seconds. The top of his hat was at least two inches from his head when this fiery lance pierced him. . . . He experienced a peculiar burning or stinging sensation of the scalp for several hours afterwards."

The telegraph wires and the buildings were struck by lightning on several occasions. When a flash struck the telegraph wire on July 19, 1884, for a moment the line resembled a belt of fire, and vibrated violently for some minutes after the discharge. Frequent discharges have also been observed between the ground wire and the rocks on which it rested.

On August 12, 1879, it is recorded:—"At 5.40 p.m. a bolt of lightning went through the arrester with the report of a rifle, throwing a ball of fire across the room against the stove and tin sheathing. At 6.35 p.m. the lightning struck the wire and building at the north end where the wires come through the window and arrester, with a crash equal to any 40-pounder. It burned every one of the four wires coming in at the window into small pieces, throwing them with great force in every direction, and filled the room with smoke from the burning gutta-percha insulation. The window-sash was splintered on the outside, one pane of glass broken, and another coated with melted copper. The anemometer wires were also burnt up, and the dial burned and blown to pieces." Barometer bulbs were cracked by lightning on August 21, 1881; and on August 15, 1886, it is recorded:—"Station struck by lightning at 6.45 p.m.; shattered the west window of the dining-room, breaking four panes of glass and shivering the casing, leaving an opening between the casing and the wall; also slightly damaged the building in several places, and set fire to some articles in the storehouse, and burned several holes in the side of a tin bucket, allowing the water in it to escape."

Again, on September 7, 1883, we read:—"Lightning struck the anemometer cups, burning a round hole about half an inch in diameter in one of them. The contact spring in the dial was badly bent, and the point of contact was considerably damaged by melting. When the insulated wire passed over a nail in the side of the house, the head of the nail was melted and the wire burnt off. Inside the window, at a bend in the wire, electricity passed off into the sill, setting some paper on fire. The paper covering the battery box was ignited. Three window lights were broken. A tourist in the dining-room was badly stunned. Observer in passing from dining-room to office was severely stunned by what seemed and felt like a blow on the head. One hand swelled rather badly. The report in the house was double, and sounded like striking red-hot iron upon which cold water had just been thrown."

Some interesting observations of hail-stones are also given. The stones are said to vary in size from peas to pigeons' eggs, and many of them were conical in shape. Sometimes they consisted of soft white snow throughout, without any nucleus, and at other times they were so hard as to require a heavy blow to break them. When this was the case, the broken hail-stones presented a stratified structure, with a centre of clear ice, and concentric rings of solid and spongy ice, with an outer covering of soft snow. It is noted that in all hail-storms the fall of hail entirely ceased for about half a minute following a heavy electric discharge; after this interval, however, the fall was considerably heavier than before.

The following observations, made on October 12, 1877, have an important bearing on the subject of hail formation:—"The rotatory movement of the hail cloud could be plainly seen, and with every violent flash of lightning the passing cloud would grow perceptibly darker, indicating increased condensation. The hail formed by this cloud must have fallen about three miles below, for the wood-packers reported quite solid hail at timber line, and none above. This verifies the theory that a hail cloud can be transported laterally several miles while the ice stones are forming."

The constant crackling of hail when it reaches the earth is also referred to, and rocks are said to give forth a peculiar chattering noise, as if they were shaken by subterranean convulsions, during the occurrence of heavy hail-storms.

These instances of inductive actions manifested during thunderstorms, electrical discharges, and their relation to hailstorms might be considerably multiplied. They confirm previous observations in an intense manner, and should be of some assistance to the student of meteorological phenomena.

R. A. GREGORY.

NOTES.

THE ordinary general meeting of the Institution of Mechanical Engineers will be held on Wednesday evening, October 29, and Thursday evening, October 30, at 25 Great George Street, Westminster, by permission of the Council of the Institution of Civil Engineers. The chair will be taken at half-past seven p.m. on each evening, by the President, Mr. Joseph Tomlinson. The following papers will be read and discussed, as far as time permits:—On tube-frame goods waggons

of light weight and large capacity, and their effect upon the working expenses of railways, by Mr. M. R. Jefferds, of London (communicated through Mr. Henry J. Marten). In connection with this paper the members are invited to inspect one of the waggons which will be on view at any time during daylight on October 29 and 30, at the Victoria passenger station of the London, Chatham, and Dover Railway, where it will be standing in the siding behind the main arrival platform, by permission of the railway company. On milling cutters, by Mr. George Addy, of Sheffield. On the mechanical treatment of moulding sand, by Mr. Walter Bagshaw, of Batley.

THE annual general meeting of the Mineralogical Society will be held in the apartments of the Geological Society, Burlington House, Piccadilly, on Tuesday, November 11, at 8 p.m.

AN Exhibition, for the most part national, will be held at Lyons in 1892. With regard to electricity it will be international.

M. FLAHAULT, the eminent algologist and Professor of Botany at Montpellier, has been sent by the Minister of Public Instruction in France on a mission to Sweden, Norway, and Denmark, for the purpose of endeavouring to establish permanent relations between the Scandinavian Universities and the centres of higher instruction in France.

THE municipality of Verona gave a cordial reception to the Italian Botanical Society, which held its third annual Congress in that city from September 1-8. Prof. Arcangeli was elected President of the Society in the place of Prof. Caruel.

THE Vienna Academy commissioned Dr. G. Bukowski to make some geological investigations in Western Asia Minor at the beginning of the present year. After leaving the Khonas-Dagh, Dr. Bukowski made an excursion from Denisli to Tshoekelez-Dagh, the district lying to the north of the Tshuruksu Valley; from thence he proceeded over the Tshukur Pass to Jorengume, and over the Davas Ovassi table-land to the foot of the Baba-Dagh. At the end of June his researches were interrupted by ill-health.

THE Geologists' Association will open its winter session with a *conversazione*, which will be held in the Library of University College, Gower Street, on Friday, November 7. Many objects of geological interest will be exhibited on the occasion.

MR. JOHN HANCOCK, the well-known naturalist, died at his residence at Newcastle-on-Tyne on Saturday, at the age of 84. Mr. Hancock was an admirable observer of bird-life, and the Museum of the Natural History Society at Newcastle has profited largely by his knowledge and enthusiasm as an ornithologist.

THE death is announced, at the age of seventy-six, of Dr. Wenzel Leopold Gruber, the eminent Professor of Anatomy at the University of St. Petersburg.

THE first series of lectures provided by the Sunday Lecture Society begins on Sunday afternoon, October 19, in St. George's Hall, Langham Place, at 4 p.m., when Prof. Silvanus P. Thompson will lecture on "Waves of Light," with illustrations and experiments. Lectures will subsequently be given by Dr. B. W. Richardson, Mr. A. Elley Finch, Dr. Andrew Wilson, Mr. Willmott Dixon, Mr. Arthur Nicols, and Sir A. C. Lyall.

A SCIENTIFIC expedition, under the auspices of the Field Naturalists' Club of Victoria, will start on or about November 15 for the Eastern Islands. Its work will occupy from ten to fourteen days. The expedition will be divided into two parties, one of which will land on Deal Island, the other on Flinders Island. A sub-committee appointed to make the necessary arrangements has reported that while there is little information with regard to Deal Island, the utility of visiting one of the Kent group, which are small, lies in the possible opportunity of determining the limits of the strictly Tasmanian and Australian fauna and flora, since the islands lie considerably nearer the Victorian than the Tasmanian coast. The greater number of members will probably proceed to Flinders Island. They will, in all probability, be able to visit Barren Island, which lies close to the southern portion of Flinders Island. Two varieties of wallaby, waterfowl, and game of various kinds appear to be plentiful, and the nature of the country seems favourable for the pursuit of different branches of natural history.

THE October number of the *Kew Bulletin* begins with a paper on an edible fungus of New Zealand (*Hirneola polytricha*, Montagne). In order that the value of this fungus as an article of food might be tested, a supply of it was recently obtained for Kew. A portion of this supply was submitted for analysis to Prof. Church, F.R.S., and a note by him on the subject is printed in the *Bulletin*. Other subjects dealt with are Mexican Fibre or Istle, a forest plague in Bavaria (*Liparis Monacha*), okro fibre (*Hibiscus esculentus*, L.), cocoa-nut butter, and soil and cultivation in Yoruba-land.

IN the one hundred and third Annual Report of the Royal Botanic Garden, Calcutta, Dr. King says that the attention of the staff during the past year was devoted chiefly to the maintenance, in as high a state of efficiency as possible, of the various departments of the garden. Special attention was given to the herbarium, and a considerable number of new species were described. The sum of 1000 rupees having been granted in order that specimens might be obtained in Burma and Assam, Dr. King was enabled to do more than usual in these provinces. Under a recent order of the Government of India this exploration will be extended. An official document relating to Dr. King's Report, and issued by order of the Lieutenant-Governor of Bengal, contains the following passage:—"The control of Indian botanical operations has been centralized in the Calcutta Gardens, and the Superintendent has been appointed Director of the Botanical Survey of India. The grants promised by the Administrations of Burma and Assam will enable collections to be made on a larger scale and more continuously. As this work will constitute a separate Department, it has been ordered that in future years a separate Report should be submitted on the subject."

THE twenty-eighth Annual Report of the Government cinchona plantation and factory in British Sikkim, by Dr. King, has been issued. At the end of the financial year 1889-90 the plantation consisted of 4,682,401 trees of various ages, and of a nursery stock amounting to 264,000 seedlings. The crop collected during the year amounted to 304,705 pounds. The products of the factory were 1833½ pounds of sulphate of quinine, and 6578 pounds of febrifuge. The whole of the quinine and the greater part of the febrifuge were manufactured by the new fusel-oil process; and, as the arrangements for working this process were quite completed during the year, the old acid and alkali method of manufacture has now been definitively abandoned. An additional year's experience of the fusel-oil process confirms Dr. King's previously expressed opinion of its complete success. The quinine turned out by it is of excellent appearance and great purity, in the latter respect comparing favourably with most of the brands of the drug of European manufacture.

THE National Association for the Promotion of Technical and Secondary Education have issued some valuable "notes" on the working of the Technical Instruction Act; and a series of "suggestions" to County Councils and other local authorities on the use of the new fund allocated to County Councils for the purposes of technical and secondary education.

The greater part of the new number of the *Mineralogical Magazine* consists of a most careful and interesting paper, by Mr. L. Fletcher, on the Mexican meteorites, with especial regard to the supposed occurrence of wide-spread meteoritic showers. The number includes also the following papers, all of which are short:—A visit to the calcite quarry in Iceland, by J. L. Hoskyns-Abrahall; sanguinite, a new mineral, and Krennerite, by H. A. Miers; notes on Bowenite, or pseudo-jade from Afghanistan, by Major-General C. A. McMahon; on the relations between the gliding planes and the solution planes of augite, by Prof. J. W. Judd.

It is reported from India that Mr. John Eliott, Meteorological Reporter to the Government, starts this month on tour, and will first visit Quetta and then go down the Indus to Kurrachee and Bombay, and finally make his way to Calcutta.

We learn from *La Nature* that on September 21 Marseilles was visited by a severe thunderstorm accompanied by torrential rain and hail. The storm began about 6 a.m., and lasted 2½ hours. Everything that was in front of the shops was carried away, and the port was filled with *débris* of all kinds. Many of the hailstones were of the size of walnuts and even of fowl's eggs; several places were struck by lightning, and many animals were drowned. Such atmospheric disturbances are said to be very rare at Marseilles.

The Agricultural Department of Bohemia has published, in a quarto volume of 138 pages, the results of the rainfall observations made in that country during the year 1889, in continuation of the work formerly undertaken by the Hydrographic Committee, under the direction of Dr. Studnička. The stations now number 707; the rainfall is measured at 6 a.m. daily, and the amount set down to the previous day. For a large number of stations the daily rainfall is entered in the tables. The yearly results are shown upon a map, by means of reference numbers to the tables and curves for each 100 mm., and the various watersheds are also shown by red outlines. The tables show the number of wet days, and the maximum daily falls at each station. This rainfall service is now one of the most complete in Europe.

The Annual Report of the Director of the Royal Alfred Observatory, Mauritius, for the year 1888 shows that the temperature of the air was $0^{\circ}\cdot5$ below the average of the last fourteen years, and below the average in every month except August and November. The greatest rainfall in one day was 4·5 inches, on March 19, on which day 1·3 inch fell in fifty minutes. The island has not been visited by a hurricane since March 21, 1879, but the Observatory continues its useful work of examining the logs of ships traversing the Indian Ocean, and synoptic charts have been prepared for eighteen days on which tropical cyclones were experienced. The upper clouds, when visible, generally travelled from the westward. The number of unusual sky glows was less than in 1887, but were observed in all months except September, October and December. There seems to be some connection between mortality from fever and rainfall; the maximum mortality occurs about two months after the maximum rainfall, and the minimum mortality about two months after the minimum rainfall. The report contains monthly rainfall values for eight stations, and results of the meteorological observations at Seychelles and Rodrigues.

Four interesting phenological maps of Finland appear in the *Meteorologische Zeitschrift* for September. In these, Dr. Ihne shows the date of flowering of *Ribes rubrum*, *Prunus padus*, *Syringa vulgaris*, and *Sorbus Aucuparia* in different parts of the country, by a series of zones, embracing each five days. *Ribes* and *Prunus* begin to flower earlier than the two others, and, accordingly, the zone for a given date is further north in the case of the former; their maps also present more zones. The isophanes (or lines of the same date of flowering), bordering the zones, are approximately parallel to the parallels of latitude. The regions from June 9 to 20 have more regular boundaries than those from May 26 to June 4, more equable weather having then set in. The presence of ice, and its cooling effect on water (even after melting), and so on wind, delays the time of flowering. Thus it is that islands and the land north-west of Lake Ladoga, &c., show retardation. The unequal breadth of the zones is remarkable; Dr. Ihne supposes the cause to lie in an irregular progression of the wave of heat, due to the arrangement of land and water.

A report from Nicaragua states that an earthquake occurred at Granada on September 30. No damage was done, nor did any volcanic eruption of the Mombacho take place.

A gigantic pendulum has been suspended from the centre of the second platform of the Eiffel Tower. It consists of a bronze wire, one hundred and fifteen metres long, with a steel globe weighing ninety kilogrammes at the end. The object is to demonstrate visibly the motion of the earth.

In the course of archæological explorations lately carried on in the Crimea, Prof. Vasselovski found painted human bones in two graves—six skeletons in one grave, and one in another. Prof. Grempler, of Breslau, is of opinion that these graves belonged to the original inhabitants of the Crimea, the Cimmerians of Herodotus. They laid their dead on elevated spots, so that the birds might consume the flesh. When quite bleached, the skeletons were painted with some mineral pigment. Several graves containing such painted skeletons have been found in Central Asia. Only three had been previously found in the Crimea.

We learn from the *Botanical Gazette* that the Cornell University Experiment Station is making a large and important collection of cultivated plants; collectors being sent to leading nurseries, and botanists employed in many parts of the country to collect the cultivated plants.

The rich algological herbarium collected by the late Prof. F. Hauck, of Trieste, has been purchased by Mme. Weber van der Bosse, of Amsterdam.

The *Victorian Naturalist* learns from Mr. Tisdall that the English foxglove has established itself on the slopes of the Stringer's Creek Valley, near Walhalla. Last season in some parts the banks were purple with them.

Nautilus shells are being picked up by fortunate hunters at Portland, Victoria. The *Portland Guardian* says the search after the shells is very keen, and that before daylight numbers of enthusiasts visit the beaches ready to prosecute their searches as soon as the morning breaks.

The Museum Committee of the Leicester Town Council, in their twelfth Report, just issued, are able to give a most satisfactory account of the institution under their charge. The building of the Town Museum has lately undergone extensive repairs, and many important additions have been made to the various departments. We may note that a very ingenious method for the exhibition of coins is in use. The pulling of a lever rotates a frame—containing cards in which the coins are inserted—in such a manner that the obverse and reverse, with a full description of each coin, are shown at the will of the observer. This method has been devised by Mr. Montagu Browne, the Curator.

A valuable paper, by Mr. E. Wilson, on fossil types in the Bristol Museum, has been reprinted from the *Geological Magazine* for August and September 1890. The Bristol Museum, it seems, contains 186 distinct fossil forms; and many of them "possess for the student of British palæontology a very high interest, not only on account of the remarkable nature of the fossils themselves, but also from the fact of their having been described by some of the most distinguished of palæontologists."

MESSRS. CASSELL AND CO. have issued Part 24 of their "New Popular Educator." It includes a coloured map of France.

FOUR new parts of the "Encyklopædie der Naturwissenschaften" (Breslau, E. Trewendt) have been issued. In Parts 58 and 59 of the second Abtheilung some important contributions are made to the dictionary of chemistry included in this great work. Parts 5 and 6 of the third Abtheilung contain portions of a hand-book of physics.

THE preliminary surveys for the projected Onega-White Sea Canal have been completed. The British Vice-Consul at Archangel in his last Report says that the following facts have been established. The level of the White Sea is about 15 feet higher than that of the Lake Onega; and the length of the proposed canal would be 219 versts, of which 129 versts are a natural waterway. The proposed measurements of the canal are—breadth, 63 feet; at the locks 112 feet; and along its other portions the proposed depth is 10 feet. The cost is estimated at about 7,500,000r. (£800,000), not including the expenses incurred in the construction of a port at a point on the coast of the White Sea. With the construction of the canal it is expected that the cost of transport of goods from St. Petersburg to Archangel will be diminished from 1r. per poud to 40c. The canal will afford every facility for the transport of fish from the plentiful fishing-grounds of the White Sea to St. Petersburg, and also for the transport of the mining products of Olonets. It will also be of great strategical importance in connecting St. Petersburg and Cronstadt with the White Sea. There can be no doubt, the Vice-Consul thinks, that, considering the unlimited supply of timber in the province of Olonets, and the enterprising character of the population, shipbuilding will be carried on on a large scale when the canal is constructed.

IF we were to judge by statistics alone, we should be forced to conclude that the present system of granting rewards for the destruction of wild animals in India has had little or no effect in diminishing their numbers or in decreasing the mortality caused by them. This conclusion, however, would not be in accordance with facts. The methods according to which the statistics are collected have been so much improved that no induction can safely be made from the figures available. This is pointed out in a recent Report of the Revenue Department of the Government of Madras. The Report continues:—"The experience of almost every District officer who has been some years in the country would be that the number of destructive wild animals had largely decreased with the advance of cultivation and the progress of railways, and the evidence of natives would probably be the same. There are parts of the country still where, owing to the existence of forest and difficulty of access, wild animals of prey continue to exist in large numbers, and it is the case that, owing to various causes, Europeans at all events do less now in the way of killing large game than formerly was the case. They have less time to spare from their official duties, and less money to spend. It can hardly, however, be doubted that, owing to the existence of the system of granting rewards for animals slain, native shikaris are encouraged to maintain a profession which otherwise probably they would give up from want of support, and for this reason, if for no other, the Board would not wish to see at present any change made in the system of granting rewards. It may be hoped that the construction of the East Coast Railway, and the branch from it through the heart of the Vizagapatam district to the Central Provinces, will tend in a great measure to reduce the number of wild animals in the districts where they now do very considerable damage. Cultivation and population in tracts now given up to jungle and grass will increase largely, and the need of wood for the railways will lead probably to the destruction of large areas of jungles, which now exist in tracts which should be devoted to agriculture."

NATURALISTS will read with interest a paper in *Humboldt* for September, in which Prof. Forel, of Zürich, gives the results of a visit he lately paid to Tunis and Eastern Algeria, chiefly to observe the ants there. Looking from a ship at the dreary grey wastes, and the large date-palm oasis of Gabes, one fancies all animal life must be concentrated under the palms. But really there is very little of it there, and hardly anything singular; while the sand of the desert contains, round each of the poor, small, sparse plants, a host of beetles and other insects, many of them with striking adaptations and peculiarities. Some live on excrement of camels, asses, &c., some on the plants, and some prey on other animals, big and small. In one ant-hill he found that several ants had a small brown object clinging to the lower part of an antenna; in some cases, one on either antenna. On examination, this fell off, and was found to be a small beetle, which evidently clings there as guest; it has tufts of hair, which are probably licked by the ant. The host did not seem to trouble itself about this little creature, which, by its odd post, is enabled to accompany the ant in its wanderings and changes of abode. Prof. Forel remarks on the peaceful character of the ants in that region; with few exceptions they avoid fighting, and only one ant was found capable of piercing the human skin.

THE phenomenon of globular lightning was imitated by M. Planté, it will be remembered, with his secondary batteries. It has been recently shown by Herr von Lepel (*Met. Zeits.*) that this can also be done with so-called statical electricity, obtained from an influence-machine. Two thin brass-wire points from the poles of a powerful machine being held at a certain distance from the opposite sides of an insulated plate of mica, ebonite, glass, or the like, there appear small red luminous balls, which move about, now quickly, now slowly, and are sometimes still. Even better effects were had with a glass or paper disk which had been sprayed with paraffin. Small particles of liquid or dust seem to be the carriers of the light. A slight air-current makes the spherules disappear with hissing noise. These spherules, the author remarks, are phenomena of weak tension; an increase of the tension gives a rose spark-discharge. Various interesting analogies with globular lightning are traced.

IN a long series of articles a native Japanese paper gives some interesting figures about the students of Tokio. There are 107,312 students in the whole Empire in the various colleges and other high schools (primary schools and ordinary middle schools excepted). Of this number, 38,114 represent students prosecuting their studies in the capital—that is to say, about 40 per cent. of the whole number are congregated in Tokio. Among the 38,114 students, 6,899 are domiciled in Tokio, so that the number of those coming from other localities is 31,215. The amounts which individual students spend vary from seven or eight dollars to about fifteen dollars per month. Taking the average, it may be assumed that each student spends ten dollars a month, or 120 dollars a year. Thus the total amount of money annually disbursed by these lads is a little over 3,700,000 dollars. In other words, money aggregating over three millions and a half is being yearly drawn from the provinces to the capital through this channel. The provinces receive little in return, for few of the students ever go back to their homes, their sole ambition being to remain in the capital, and there rise to eminence in some walk of life.

THE British Consul at St. Jago de Cuba, in his latest Report, refers to the disease in the cocoa-nut plantations there, and the result of the investigations into the pest made by the Academy of Sciences of Havanna. Their Report attributed the disease to a microscopic fungus of the genus *Uredo*, and stated that the

only remedy was to cut down and burn all the trees attacked. Dr. Galves, however, endeavoured at the time to convince the Academy that this was an error, and that the disease proceeded from an hemipterous insect of the genus *Coccus*, which he classified as *Diaspis vandalicus*, and afterwards Dr. Valdés Dominguez, of Baracoa, confirmed this opinion. Finally, at the end of last year, Dr. Carlos de la Torre, a member of the Academy, set all doubt at rest, and proved that the *Uredo* referred to by the Commission did not even exist in the cocoa-nut trees, and that the small stains which had been mistaken for it were normal to the plant, and existed both in the healthy and attacked trees, and that the real cause was the *Diaspis vandalicus* of Galves, together with three other species of *Coccus*. The first symptom of the disease is the appearance on the under side of the leaflets of the fronds, of small white stains, almost imperceptible. These soon attain the size of a pepper-corn, and impart a general white colour to the leaflets, which change, later on, to yellow, and finally dry up.

THE additions to the Zoological Society's Gardens during the past week include two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by Captain C. Crawford-Caffier, R.N.; an African Civet Cat (*Viverra civetta*), a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Lieut.-Colonel W. Gordon Pachett, W.I.R.; a Serval (*Felis serval*) from West Africa, presented by Mr. J. H. Cheetham, F.Z.S.; two Long-fronted Gerbilles (*Gerbillus longifrons* ♂ ♀) from Western Asia, presented by Mrs. F. A. Kitchener; two Blackcaps (*Sylvia atricapilla*), a Garden Warbler (*Sylvia hortensis*), British, presented by Mr. J. Young, F.Z.S.; three Passerine Parrots (*Psittacula passerina*) from Brazil, presented by Mr. Arthur Robottom; a Barnard's Parrakeet (*Platycercus barnardi*) from Australia, presented by Mrs. E. M. Temple; a Golden Eagle (*Aquila chrysaëtus*) from Morocco, presented by Mr. Charles A. Payton; a Snowy Egret (*Ardea candidissima*) from America, presented by Mr. H. H. Sharland; a Herring Gull (*Larus argentatus*), three Lesser Black-backed Gulls (*Larus fuscus*), British, presented by the Hon. J. S. Gathorne Hardy, M.P., F.Z.S.; two Purple Porphyrios (*Porphyrio cæruleus*) from Sicily, presented by Mr. J. I. S. Whitaker; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mrs. Wanklyn; two North African Jackals (*Canis anthus*) from North Africa, deposited; two Philantomba Antelopes (*Cephalophus maxwelli* ♂ ♀) from South Africa, three Passerine Parrots (*Psittacula passerina*) from Brazil, a Lucian's Parrakeet (*Palæornis luciania* ♂) from China, purchased; six Esquimaux Dogs (*Canis familiaris* var. 4 ♂ 2 ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on October 16 = 23h. 41m. 28s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4940	—	—	23 15 9	+ 7 28
(2) G.C. 4964	—	Bluish-green.	23 20 36	+41 47
(3) 280 Schj.	8	Very red.	23 55 39	+59 44
(4) ψ_1 Aquarii	4·5	Whitish-yellow.	23 9 34	− 9 45
(5) ψ_3 Aquarii	5·5	White.	23 12 43	−10 16
(6) V Tauri	Var.	Reddish.	4 45 40	+17 21
(7) R Tauri	Var.	Very red.	4 22 16	+ 9 55
(8) R Lyræ	Var.	Red.	18 52 0	+43 48

Remarks.

(1) The spectrum of this nebula has not yet been recorded. It is described as "considerably bright; pretty small; round; pretty suddenly brighter in the middle."

(2) This remarkable planetary nebula was stated by Lassell to be bi-annular, consisting of a nucleus and two rings, whilst Lord Rosse observed a spiral structure. Herschel described it as "a very remarkable planetary nebula; very bright; pretty small; round; blue." The spectrum is also a remarkable one, consisting of the three ordinary lines of the nebulæ, and, in addition, a line near wave-length 470, which was first seen by Dr. Huggins. This line occurs only in this nebula, with the exception of the Orion nebula, where it has been recorded by Mr. Taylor, and is also feebly impressed in Mr. Lockyer's photograph of the spectrum. It is, however, far brighter in the planetary nebula than in the Orion nebula. It has been suggested that the line is really the blue band of carbon under such conditions that most of the light is concentrated at about wave-length 470, as is sometimes the case in the laboratory. If this be so, it ought not to be so sharply defined as the other lines, and an observation should be made with reference to this point. Other lines, especially near the carbon flutings in the green, should also be looked for.

(3) The spectrum of this star, according to the observations of Dunér, is undoubtedly a banded one, but whether it is one of Group II. or Group VI. is doubtful. The strongest band is in the red, and this is very little degraded. In the green there is another band, which is wider but not so strong. With less certainty other bands were detected in the red and yellow-green, and another "very vaguely" in the blue. The spectrum is well worth further investigation, as we are likely to learn most by a study of the apparent departures from the regular types.

(4) A star of the solar type, with fine lines (Konkoly). The spectrum should be further examined as to whether the temperature is increasing (Group III.) or decreasing (Group V.). The fineness of the lines tend to show that it is the latter.

(5) A star of Group IV. (Konkoly).

(6) The spectrum of this variable, according to Gore's Catalogue, has yet to be determined, and the forthcoming maximum of October 19 may therefore be taken advantage of. The period is about 169 days, and the magnitude ranges from 8·3–9·0 at maximum to $<$ 12·8 at minimum.

(7) This variable of Group II. will reach a maximum about October 21. The period is about 326 days, and the variation from 7·4–9·0 at maximum to $<$ 13 at minimum. Bright lines and carbon flutings should be looked for.

(8) This well-known variable of Group II. will reach a maximum about October 24. The range is small (4·3–4·6) and the period short (46 days), two conditions which appear to go together, exactly as is demanded by the collision theory of this kind of variability. Further, if that explanation be correct, it is not likely that bright lines will appear at maximum, and this may be made a test observation. A. FOWLER.

THEORY OF SOLAR RADIATION.—Mr. W. Goff has written a pamphlet in which he propounds a theory of the sun's radiation of heat. It is well known that geologists and physicists demand a much longer duration of the sun's past activity than the present estimate of the expenditure of heat would allow, supposing that there have been no unknown means of supply. Dr. Croll, in "Stellar Evolution," brings forward evidence in support of the longer periods. To account for the great disparity that exists between the results arrived at from different points of view, he assumed that the primitive nebulous mass possessed a store of energy derived from the impact of two large cold bodies moving with enormous velocities. Mr. Goff also thinks that the grounds upon which geologists and biologists found their conclusions are more certain and trustworthy than those of the physicist. He does not, however, supplement gravitational energy by energy derived from other sources, in order to account for the sun's outlay, but shows that the methods adopted for arriving at values of the amount are at fault. In his words:—"Radiant energy is a very different thing from absorbed heat, and I have endeavoured to demonstrate that its value must be considerably less. Also I have shown that the current estimates of the sun's annual loss of heat are founded entirely on an absorbed heat basis. They must, consequently, if my arguments are correct, be far in excess of what his expenditure actually is." The distinction between radiant energy and absorbed heat is clearly indicated, and it is evident that, unless the value of each is the same, the present determinations of the sun's emission of heat must be incorrect.

THE SATELLITES OF SATURN.—The micrometer measures of the satellites of Saturn made by Dr. Hermann Struve with the 30-inch Pulkova equatorial, has led to some interesting and im-

portant results. In a recent communication (*Astronomische Nachrichten*, No. 2983), the orbits of Mimas and Enceladus, and their relation to those of the other satellites, are considered. The orbit of Mimas has an eccentricity of 0·016, and an inclination of 1° 26′. The retrograde movement of the nodes, is about 1° per day, and is accompanied by a direct movement of the perisaturnium point, which is almost equal to it (− 365° and + 371° per year). The comparison of the Pulkova observations with those made at Washington (1882–86) indicates an acceleration of the mean motion of Mimas, which corresponds to a retardation in the mean movement of Tethys. Dr. Struve shows that the changes in the elements and mean motions of the two satellites may increase indefinitely, or vary between certain limits. The latter explanation is proved to be the correct one, and a discussion of the observations of Sir W. Herschel, Lassell, Marth, Newcomb, Asaph Hall, &c., leads to the conclusion that the conjunctions of Enceladus and Dione occur at the perisaturnium of the former satellite or nearly so, whilst those of Mimas and Tethys oscillate 45° about the point midway between the ascending nodes of their orbits on Saturn's equator, and perform this libration in about sixty-eight years.

The masses of Dione and Tethys inferred by Dr. Struve from the libration are respectively seven and eleven times smaller than those deduced from photometric comparisons with Titan. The result for Mimas is twenty-two times smaller than that furnished by photometry. It appears necessary to admit, therefore, that in the system of Saturn, as in that of Jupiter, either the intrinsic brilliancy of the satellites increases, or their density decreases, as the planet is approached. A knowledge of the masses of the four above-named satellites, determined photometrically and found by Dr. Struve, allows those of Enceladus and Rhea to be estimated with some probability. The following are the calculated and the hypothetical values in terms of the mass of Saturn : Mimas, 1/11,500,000 ; Enceladus, 1/4,000,000 ; Tethys, 1/767,000 ; Dione, 1/528,000 ; Rhea, 1/200,000 ; Titan, 1/4700. By adopting the above hypothetical values of the masses of Enceladus and Rhea, the observed and calculated values of the secular motions of the nodes and apses are found to agree in a very satisfactory manner. The spheroidal constant of Saturn has been determined as 0·0258, which differs considerably from the value 0·0223 assumed in a previous paper (*Astronomische Nachrichten*, No. 2946). This alteration obviates the necessity of giving the ring-system a sensible mass in the calculations.

A NEW COMET (*d* 1890).—A faint comet was discovered by Mr. E. E. Barnard, of the Lick Observatory, on the 6th inst. It was then situated in Capricornus.]

ANTARCTIC EXPLORATION.

THE following address, on "The Objects of Antarctic Exploration," was delivered at the annual meeting of the Bankers' Institute of Australasia, at Melbourne, on Wednesday, August 27, by Mr. G. S. Griffiths, F.G.S., F.R.G.S., His Excellency the Earl of Hopetoun being in the chair.

Mr. Griffiths said,—My experience, during the four years which have elapsed since this project was first mooted in Melbourne, is that any reference to the subject is sure to be met with the query, *Cui bono?* What good can it do? What benefit can come from it? What is the object to be served by such an expedition?

In setting myself to the task of answering these questions, let me observe that it would indeed be strange if an unexplored region, 8,000,000 square miles in area—twice the size of Europe—and grouped around the axis of rotation and the magnetic pole, could fail to yield to investigators some novel and valuable information. But when we notice that the circle is engirdled without by peculiar physical conditions which must be correlated to special physical conditions within, speculation is exchanged for a confident belief that an adequate reward must await the skilled explorer. The expected additions to the geography of the region are, of all the knowledge that is to be sought for there, the least valuable. Where so many of the physical features of the country—the hills, the valleys, and the drainage lines—have been buried beneath the snow of ages, a naked outline, a bare skeleton of a map, is the utmost that can be delineated. Still, even such knowledge as this has a distinct value, and as it can be acquired by the explorers as they proceed about their more important researches, its relatively small value ought not to be admitted as a complete objection to any enterprise which has other objects of importance. Our present acquaintance with the geography of the region is excessively limited. Ross just viewed the coasts of Victoria Land, between 163° E. and 160° W. long. ; he trod its barren strand twice, but on each occasion for a few minutes only. From the adjacent gulf he measured the heights of its volcanoes, and from its offing he sketched the walls of its icy barrier. Wilkes traced on our map a shore-line from 97° E. to 167° E. long., and he backed it up with a range of mountains, but he landed nowhere. Subsequently Ross sailed over the site assigned to part of this land, and hove his lead 600 fathoms deep where Wilkes had drawn a mountain. He tells us that the weather was so very clear, that had high land been within 70 miles of that position he must have seen it ("Ross's Voyage," 1278). More recently Nares, in the *Challenger*, tested another part of Wilkes's coast-line, and with a like result ; and these circumstances throw doubts upon the value of his reported discoveries. D'Urville subsequently followed a bold shore for a distance of about 300 miles from 136° E. to 142° E. long. ; whilst in 67° S. lat., and between 45° E. and 60° E. long., are Enderby's and Kemp's lands. Again, there is land to the south of the Horn, which trends from 45° to 75° S. lat.. These few discontinuous coast-lines comprise all our scanty knowledge of the Antarctic land. It will be seen from these facts that the principal geographical problem awaiting solution in these regions is the interconnection of these scattered shores. The question is, Do they constitute parts of a continent, or are they, like the coasts of Greenland, portions of an archipelago, smothered under an overload of frozen snow, which conceals their insularity? Ross inclined to the latter view, and he believed that a wide channel leading towards the Pole existed between North Cape and the Balleny Islands ("Ross's Voyage," 1221). This view was also held by the late Sir Wyville Thomson. A series of careful observations upon the local currents might throw some light upon these questions. Ross notes several such in his log. Off Possession Island a current, running southward, took the ships to windward (*ibid.*, 1195). Off Coulman Island another drifted them in the same direction, at the rate of eighteen miles a day (*ibid.*, 1204). A three-quarter knot northerly current was felt off the barrier, and may have issued from beneath some part of it. Such isolated observations are of little value, but if they were multiplied, and were the currents correlated with the winds experienced, the information thus obtained might enable us to detect the existence of straits, even where the channels themselves are masked by ice-barriers.

Finally, it is calculated that the centre of the polar ice-cap must be three miles, and may be twelve miles, deep, and that, the material of this ice mountain being viscous, its base must spread out under the crushing pressure of the weight of its centre. The extrusive movement thus set up is supposed to thrust the ice cliffs off the land at the rate of a quarter of a mile per annum. These are some of the geographical questions which await settlement.

In the geology of this region we have another subject replete with interest. The lofty volcanoes of Victoria Land must present peculiar features. Nowhere else do fire and frost divide the sway so completely. Ross saw Erebus belching out lava and ashes over the snow and ice which coated its flanks. This circumstance leads us to speculate on the strata that would result from the alternate fall of snow and ashes during long periods and under a low temperature. Volcanoes are built up, as contradistinguished from other mountains, which result from elevation or erosion. They consist of *débris* piled round a vent. Lava and ashes surround the crater in alternate layers. But in this polar region the snowfall must be taken into account as well as the ash deposit and the lava-flow. It may be thought that any volcanic ejecta would speedily melt the snow upon which they fell, but this does not by any means necessarily follow. Volcanic ash, the most widespread and most abundant material ejected, falls comparatively cold, cakes, and then forms one of the most effective non-conductors known. When such a layer, a few inches thick, is spread over snow, even molten lava may flow over it without melting the snow beneath. This may seem to be incredible, but it has been observed to occur. In 1828, Lyell saw on the flanks of Etna a glacier sealed up under a crust of lava. Now, the Antarctic is the region of thick-ribbed ice. All exposed surfaces are quickly covered with snow. Snowfalls, fish-falls, and lava-flows must have been heaping themselves up around the craters during unknown ages. What has

been the result? Has the viscosity of the ice been modified by the intercalation of beds of rigid lava and of hard-set ash? Does the growing mass tend to pile up or to settle down and spread out? Is the ice wasted by evaporation, or does the ash-layer preserve it against this mode of dissipation? These interesting questions can be studied round the South Pole, and perhaps nowhere else so well.

Another question of interest, as bearing upon the location of the great Antarctic continent, which it is now certain existed in the Secondary period of geologists, is the nature of the rocks upon which the lowest of these lava-beds rest. If they can be discovered, and if they then be found to be sedimentary rocks such as slates and sandstones, or plutonic rocks such as granite, they will at once afford us some data to go upon, for the surface exposure of granite signifies that the locality has been part of a continental land sufficiently long for the weathering and removal of the many thousands of feet of sedimentary rocks which of necessity overlie crystalline rocks during their genesis; whilst the presence of sedimentary rocks implies the sometime proximity of a continent from the surfaces of which alone these sediments, as rain-wash, could have been derived.

As ancient slate rocks have already been discovered in the ice-clad South Georgias, and as the drag-nets of the *Erebus* and the *Challenger* have brought up from the beds of these icy seas fragments of sandstones, slates, and granite, as well as the typical blue mud which invariably fringes continental land, there is every reason to expect that such strata will be found.

Wherever the state of the snow will permit, the polar mountains should be searched for basaltic dykes, in the hope that masses of specular iron and nickel might be found, similar to those discovered by Nordenskiöld, at Ovifak, in North Greenland. The interest taken in these metallic masses arises from the fact that they alone, of all the rocks of the earth, resemble those masses of extra-terrestrial origin which we know as meteorites. Such bodies of unoxidized metal are unknown elsewhere in the mass, and why they are peculiar to the Arctic it is hard to say. Should similar masses be found within the Antarctic, a fresh stimulus would be given to speculation. Geologists would have to consider whether the oxidized strata of the earth's crust thin out at the poles; whether in such a case the thinning is due to severe local erosion, or to the protection against oxygen afforded to the surface of the polar regions by their ice-caps, or to what other cause. Such discoveries would add something to our knowledge of the materials of the interior of our globe and their relation to those of meteorites.

Still looking for fresh knowledge in the same direction, a series of pendulum observations should be taken at points as near as possible to the Pole. Within the Arctic circle the pendulum makes about 240 more vibrations per day than it does at the equator. The vibrations increase in number there because the force of gravity at the earth's surface is more intense in that area, and this again is believed to be due to the oblateness of that part of the earth's figure, but it might be caused by the bodily approach to the surface at the poles of the masses of dense ultra-basic rocks just referred to. Thus, pendulum experiments may reveal to us the earth's figure, and a series of such observations, recorded from such a vast and untried area, must yield important data for the physicist to work up. We should probably learn from such investigations whether the earth's figure is as much flattened at the Antarctic as it is known to be at the Arctic.

We now know that in the past the North Polar regions have enjoyed a temperate climate more than once. Abundant seams of Palæozoic coal, large deposits of fossiliferous Jurassic rocks, and extensive Eocene beds, containing the remains of evergreen and deciduous trees and flowering plants, occur far within the Arctic circle. This circumstance leads us to wonder whether the corresponding southern latitudes have ever experienced similar climatic vicissitudes. Conclusive evidence on this point it is difficult to get, but competent biologists who have examined the floras and faunas of South Africa and Australia, of New Zealand, South America, and the isolated islets of the Southern Ocean, find features which absolutely involve the existence of an extensive Antarctic land—a land which must have been clothed with a varied vegetation, and have been alive with beasts, birds, and insects. As it also had had its fresh-water fishes, it must have had its rivers flowing and not frost-bound, and in those circumstances we again see indications of a modified Antarctic climate. Let us briefly consider some of the evidence for the existence of this continent. We are told by Prof. Hutton, of Christchurch, that 44 per cent. of the New Zealand flora is of Antarctic origin. The Auckland, Campbell, and Macquarie Islands all support Antarctic plants, some of which appear never to have reached New Zealand. New Zealand and South America have three flowering plants in common, also two fresh-water fishes, five seaweeds, three marine crustaceans, one marine mollusk, and one marine fish. Similarly New Zealand and Africa have certain common forms, and the floras and faunas of the Kerguelen, the Crozets, and the Marion Islands are almost identical, although in each case the islands are very small, and very isolated from each other and from the rest of the world. Tristan d'Acunha has 58 species of marine Mollusca, of which number 13 are also found in South America, six or seven in New Zealand, and four in South Africa (Hutton's "Origin of New Zealand Flora and Fauna"). Temperate South America has 74 genera of plants in common with New Zealand, and 11 of its species are identical (Wallace's "Island Life"). Penguins of the genus Eudyptes are common to South America and Australia (Wallace, "Dist. of Animals," 1399). Three groups of fresh-water fishes are entirely confined to these two regions. Aphritis, a fresh-water genus, has one species in Tasmania and two in Patagonia. Another small group of fishes known as the Haplochitonidæ inhabit Tierra del Fuegia, the Falklands, and South Australia, and are not found elsewhere, while the genus Galaxias is confined to South Temperate America, New Zealand, and Australia. Yet the lands which have these plants and animals in common are so widely separated from each other that they could not now possibly interchange their inhabitants. Certainly towards the equator they approach each other rather more, but even this fact fails to account for the present distribution, for, as Wallace has pointed out, "the heat-loving Reptilia afford hardly any indications of close affinity between the two regions" of South America and Australia, "whilst the cold-enduring Amphibia and fresh-water fishes offer them in abundance" (Wallace, "Dist. of Animals," 1400). Thus we see that to the north interchange is prohibited by tropical heat, while it is barred to the south by a nearly shoreless circumpolar sea. Yet there must have been some means of intercommunication in the past, and it appears certain that it took the shape of a common fatherland for the various common forms from which they spread to the northern hemisphere. As this fatherland must have been accessible from all these scattered southern lands, its size and its disposition must have been such as would serve the emigrants either as a bridge or as a series of stepping-stones. It must have been either a continent or an archipelago.

But a further and a peculiar interest attaches to this lost continent. Those who have any acquaintance with geology know that the placental Mammalia—that is, animals which are classed with such higher forms of life as apes, cats, dogs, bears, horses, and oxen—appear very abruptly with the incoming of the Tertiary period. Now, judging by analogy, it is not likely that these creatures can have been developed out of Mesozoic forms with anything like the suddenness of their apparent entrance upon the scene. For such changes they must have required a long time, and an extensive region of the earth, and it is probable that each of them had a lengthy series of progenitors, which ultimately linked it back to lower forms.

Why, then, it is constantly asked, if this was the sequence of creation, do these missing links never turn up? In reply to this query, it was suggested by Huxley that they may have been developed in some lost continent, the boundaries of which were gradually shifted by the slow elevation of the sea margin on one side and its simultaneous slow depression upon the other, so that there has always been in existence a large dry area with its live stock. This dry spot, with its fauna and flora, like a great raft or Noah's Ark, moved with great slowness in whatever direction the great earth-undulation travelled. But to-day this area, with its fossil evidences, is a sea-bottom; and Huxley supposes that the continent, which once occupied a part of the Pacific Ocean, is now represented by Asia.

This movement of land-surface-translation eastwards eventually created a connection between this land and Africa and Europe, and if when this happened the Mammalia spread rapidly over these countries, this circumstance would account for the abruptness of their appearance there.

Now, Mr. Blanford, the President of the Geological Society of London, in his annual address, recently delivered, advances matters a stage further, for he tells us that a growing acquaintance with the biology of the world leads naturalists to a belief that the placental Mammalia, and other of the higher forms of

terrestrial life, originated during the Mesozoic period, still further to the southwards—that is to say, in the lost Antarctic continent, for the traces of which we desire to seek.

But it almost necessarily follows that wherever the Mammalia were developed there also man had his birthplace, and if these speculations should prove to have been well founded we may have to shift the location of the Garden of Eden from the northern to the southern hemisphere.

I need hardly suggest to you that possibilities such as these must add greatly to our interest in the recovery of any traces of this mysterious region. This land appears to have sunk beneath the seas after the close of the Mesozoic. Now, the submergence of any mass of land will disturb the climatic equilibrium of that region, and the disappearance of an Antarctic continent would prove extremely potent in varying the climate of this hemisphere. For to-day the sun's rays fall on the South Polar regions to small purpose. The unstable sea absorbs the heat, and in wide and comparatively warm streams it carries off the caloric to the northern hemisphere to raise its temperature at the expense of ours. But when extensive land received those same heat rays, its rigid surfaces, so to speak, tethered their caloric in this hemisphere, and thus when there was no mobile current to steal northwards with it, warmth could accumulate and modify the climate.

Under the influences of such changes the icy mantle would be slowly rolled back towards the South Pole, and thus many plants and animals were able to live and multiply in latitudes that to-day are barren. What has undoubtedly occurred in the extreme north is equally possible in the extreme south. But if it did occur—if South Polar lands, now ice-bound, were then as prolific of life as Disco and Spitzbergen once were—then, like Spitzbergen and Disco, the unsubmerged remnants of this continent may still retain organic evidences of the fact in the shape of fossil-bearing beds, and the discovery of such deposits would confirm or confute such speculations as these. The key to the geological problem lies within the Antarctic circle, and to find it would be to recover some of the past history of the southern hemisphere. There is no reason to despair of discovering such evidence, as Dr. M'Cormack, in his account of Ross's voyage, records that portions of Victoria Land were free from snow, and therefore available for investigation; besides which their surface may still support some living forms, for they cannot be colder or bleaker than the peaks which rise out of the continental ice of North Greenland, and these, long held to be sterile, have recently disclosed the existence upon them of a rich though humble flora.

We have now to consider some important meteorological questions. If we look at the distribution of the atmosphere around the globe we shall see that it is spread unequally. It forms a stratum which is deeper within the tropics than about the poles and over the northern than over the southern hemisphere, so that the barometer normals fall more as we approach the Antarctic than they do when we near the Arctic. Maury, taking the known isobars as his guide, has calculated that the mean pressure at the North Pole is 29·1, but that it is only 28 at the South (Maury's "Meteorology," 259). In other words, the Antarctic circle is permanently much barer of atmosphere than any other part of the globe. Again, if we consult a wind chart we shall see that both poles are marked as calm areas. Each is the dead centre of a perpetual wind vortex, but the South Polar indraught is the stronger. Polarward winds blow across the 45th degree of north latitude for 189 days in the year, but across the 45th degree of south latitude for 209 days. And while they are drawn in to the North Pole from over a disk-shaped area 5500 miles in diameter, the South Polar indraught is felt throughout an area of 7000 miles across. Lastly, the winds which circulate about the South Pole are more heavily charged with moisture than are the winds of corresponding parts of the other hemisphere. Now, the extreme degree in which these three conditions—of a perpetual grand cyclone, a moist atmosphere, and a low barometer—co-operate without the Antarctic, ought to produce, within it, an exceptional meteorological state, and the point to be determined is what that condition may be. Maury maintained that the conjunction will make the climate of the South Polar area milder than that of the north. His theory is that the saturated winds being drawn up to great heights within the Antarctic must then be eased of their moisture, and that simultaneously they must disengage vast quantities of latent heat; and it is because more heat must be liberated in this manner in the South Polar regions than in the north that he infers a less severe climate for the Antarctic. He estimates that the resultant relative differences between the two polar climates will be greater than that between a Canadian and an English winter (Maury's "Meteorology," p. 466). Ross reports that the South Polar summer is rather colder than that of the north, but still the southern winter may be less extreme, and so the mean temperature may be higher. If we examine the weather reports logged by Antarctic voyagers, instead of the temperature merely, the advantage still seems to rest with the south. In the first place, when the voyager enters the Antarctic, he sails out of a tempestuous zone into one of calms. To demonstrate the truth of this statement, I have made an abstract of Ross's log for the two months of January and February 1841, which he spent within the Antarctic circle. To enable everyone to understand it, it may be well to explain that the wind force is registered in figures from 0, which stands for a dead calm, up to 12, which represents a hurricane. I find that during these 60 days it never once blew with the force 8—that is, a fresh gale; only twice did it blow force 7, and then only for half a day each time. Force 5 to 6—fresh to strong breezes—is logged on 21 days. Force 1 to 3—that is, gentle breezes—prevailed on 34 days. The mean wind force registered under the entire 60 days was 3·43—that is, only a four to five knot breeze. On 38 days, blue sky was logged. They never had a single fog, and on 11 days only was it even misty. On the other hand, snow fell almost every second day. We find such entries as these—"beautifully clear weather," and "atmosphere so extraordinarily clear that Mount Herschel, distant 90 miles, looked only 30 miles distant." And again, "land seen 120 miles distant, sky beautifully clear." Nor was this season exceptional, so far as we can tell, for Dr. M'Cormack, of the *Erebus*, in the third year of the voyage, and after they had left the Antarctic for the third and last time, enters in his diary the following remark. He says: "It is a curious thing that we have always met with the finest weather within the Antarctic circle; clear, cloudless sky, bright sun, light wind, and a long swell" (M'Cormack's "Antarctic Voyage," vol. i. p. 345). It would seem as if the stormy westerlies, so familiar to all Australian visitors, had given to the whole southern hemisphere a name for bad weather, which, as yet at least, has not been earned by the South Polar regions. It is probable, too, that the almost continuous gloom and fog of the Arctic (Scoresby's "Arctic Regions," pp. 97 and 137) July and August have prejudiced seamen against the Antarctic summer. The true character of the climate of this region is one of the problems awaiting solution. Whatever its nature may be, the area is so large and so near to us that its meteorology must have a dominant influence on the climate of Australia, and on this fact the value of a knowledge of the weather of these parts must rest.

To turn to another branch of science, there are several questions relating to the earth's magnetism which require for their solution long-maintained and continuous observations within the Antarctic circle. The mean or permanent distribution of the world's magnetism is believed to depend upon causes acting in the interior of the earth, while the periodic variations of the needle probably arise from the superficial and subordinate currents produced by the daily and yearly variations in the temperature of the earth's surface. Other variations occur at irregular intervals, and these are supposed to be due to atmospheric electricity. All these different currents are excessively frequent and powerful about the poles, and a sufficient series of observations might enable physicists to differentiate the various kinds of currents, and to trace them to their several sources, whether internal, superficial, or meteoric. To do this properly at least one land observatory should be established for a period. In it the variation, dip, and intensity of the magnetic currents, as well as the momentary fluctuations, of these elements, would all be recorded. Fixed term days would be agreed on with the observatories of Australia, of the Cape, America, and Europe, and during these terms a concerted continuous watch would be kept up all round the globe to determine which vibrations were local and which general.

The present exact position of the principal south magnetic pole has also to be fixed, and data to be obtained from which to calculate the rate of changes in the future, and the same may be said of the foci of magnetic intensity and their movements. In relation to this part of the subject, Captain Creak recently reported to the British Association his conclusions in the following terms. He says:—"Great advantage to the science of

terrestrial magnetism would be derived from a new magnetic survey of the southern hemisphere extending from the parallel of 40° S. as far towards the geographical pole as possible."

Intimately connected with terrestrial magnetism are the phenomena of auroras. Their nature is very obscure, but quite recently a distinct advance has been made towards discovering some of the laws which regulate them. Thanks to the labours of Dr. Sophus Tromholt, who has spent a year within the Arctic circle studying them, we now know that their movements are not as eccentric as they have hitherto appeared to be. He tells us that the Aurora Borealis, with its crown of many lights, encircles the Pole obliquely, and that it has its lower edge suspended above the earth at a height of from 50 to 100 miles, the mean of 18 trigonometrical measurements, taken with a base line of 50 miles, being 75 miles. The aurora forms a ring round the Pole which changes its latitude four times a year. At the equinoxes it attains its greatest distance from the Pole, and at midsummer and midwinter it approaches it most closely, and it has a zone of maximum intensity which is placed obliquely between the parallels of 60° and 70° N. The length of its meridional excursion varies from year to year, decreasing and increasing through tolerably regular periods, and reaching a maximum about every eleven years, when, also, its appearance simultaneously attains to its greatest brilliancy. Again, it has its regular yearly and daily movements or periods. At the winter solstice it reaches its maximum annual intensity, and it has its daily maximum at from 8 p.m. and 2 a.m., according to the latitude. Thus at Prague, in lat. 50° N., the lights appear at about 8.45 p.m.; at Upsala, lat. 60° N., at 9.30 p.m.; at Bossekop, 70° N., at 1.30 a.m. Now, while these data may be true for the northern hemisphere, it remains to be proved how far they apply to the southern. Indeed, seeing that the atmosphere of the latter region is moister and shallower than that of the former, it is probable that the phenomena would be modified. A systematic observation of the Aurora Australis at a number of stations in high latitudes is therefore desirable.

Whether or not there is any connection between auroral exhibitions and the weather is a disputed point. Tromholt believes that such a relationship is probable ("Under the Rays," 1283). He says that, "however clear the sky, it always became overcast immediately after a vivid exhibition, and it generally cleared again as quickly" ("Under the Rays," 1235). Payer declares that brilliant auroras were generally succeeded by bad weather ("Voyage of Tegelhoff," 1324), but that those which had a low altitude and little mobility appeared to precede calms. Ross remarks of a particular display "that it was followed by a fall of snow, as usual" ("Ross's Voyage," 1312). Scoresby appears to have formed the opinion that there is a relationship indicated by his experience. It is, therefore, allowable to regard the ultimate establishment of some connection between these two phenomena as a possible contingency. If, then, we look at the eleven-year cycle of auroral intensity from the meteorological point of view, it assumes a new interest, for these periods may coincide with the cycles of wet and dry seasons, which some meteorologists have deduced from the records of our Australian climate, and the culmination of the one might be related to some equivalent change in the other. For if a solitary auroral display be followed by a lowered sky, surely a period of continuous auroras might give rise to a period of continuous cloudy weather, with rain and snow. Fritz considers that he has established this eleven-year cycle upon the strength of auroral records extending from 1583 to 1874, and his deductions have been verified by others.

In January 1886 we had a wide-spread and heavy rainfall, and also an auroral display seen only at Hobart, but which was sufficiently powerful to totally suspend communication over all the telegraph lines situated between Tasmania and the China coast. This sensitiveness upon the part of the electric currents to auroral excitation is not novel, for long experience on the telegraph wires of Scandinavia has shown that there is such a delicate sympathy between them that the electric wires there manifest the same daily and yearly periods of activity as those that mark the auroras. The current that reveals itself in fire in the higher regions of the atmosphere is precisely the same current that plagues the operator in his office. Therefore, in the records of these troublesome earth-currents, now being accumulated at the Observatory by Mr. Ellery, we are collecting valuable data, which may possibly enable the physicist to count the unseen auroras of the Antarctic, to calculate their periods of activity and lethargy, and, again, to check these with our seasons. But it need hardly be said that the observations, which may be made in the higher latitudes and directly under the rays of the Aurora Australis, will have the greater value, because it is only near the zone of maximum auroral intensity that the phenomena are manifested in all their aspects. In this periodicity of the southern aurora I have named the last scientific problem to which I had to direct your attention, and I would point out that if its determination should give to us any clue to the changes in the Australian seasons which would enable us to forecast their mutations in any degree, it would give to us, in conducting those great interests of the country which depend for their success upon the annual rainfall, an advantage which would be worth, many times over, all the cost of the expeditions necessary to establish it.

Finally, there is a commercial object to be served by Antarctic exploration, and it is to be found in the establishment of a whaling trade between this region and Australia. The price of whalebone has now risen to the large sum of £2000 a ton, which adds greatly to the possibilities of securing to the whalers a profitable return. Sir James Ross and his officers have left it on record that the whale of commerce was seen by them in these seas, beyond the possibility of a mistake. They have stated that the animals were large, and very tame, and that they could have been caught in large numbers. Within the last few years whales have been getting very scarce in the Arctic, and in consequence of this two of the most successful of the whaling masters of the present day, Captains David and John Gray, of Peterhead, Scotland, have devoted some labour to collecting all the data relating to this question, and they have consulted such survivors of Ross's expedition as are still available. They have published the results of their investigations in a pamphlet, in which they urge the establishment of the fishery strongly, and they state their conclusions in the following words. They say:—"We think it is established beyond doubt that whales of a species similar to the right or Greenland whale, found in high northern latitudes, exist in great numbers in the Antarctic seas, and that the establishment of a whale fishery within that area would be attended with successful and profitable results." It is not necessary for me to add anything to the opinion of such experts in the business. All I desire to say is that if such a fishery were created, with its head-quarters in Melbourne, it would probably be a material addition to our prosperity, and it would soon increase our population by causing the families of the hardy seamen who would man the fleet to remove from their homes in Shetland and Orkney and the Scotch coasts, and settle here.

In conclusion, I venture to submit that I have been able to point to good and substantial objects, both scientific and commercial, to justify a renewal of Antarctic research, and I feel assured that nothing could bring to us greater distinction in the eyes of the whole civilized world than such an expedition, judiciously planned and skilfully carried out.

QUARTZ FIBRES.[1]

BEFORE I enter upon the subject upon which I have to address you, I wish to point out that, quite apart from any deficiency on my part which will be only too apparent in the course of the evening, it is my intention to commit two faults which may well be considered unpardonable. In the first place, I shall speak entirely about my own experiments, even though I know that the iteration of the first personal pronoun for the space of one hour is apt to be as monotonous to an audience as it is wanting in taste on the part of a lecturer. In the second place, I am going almost to depend upon the motions of a spot of light to illustrate the actions which I shall have to describe, in spite of the fact that it is impossible for an audience to get up any enthusiasm when watching the wandering motion of a spot of light the result of the manipulation of a mystery-box, of which it is impossible to see the inside. These, however, are faults which are the immediate consequence of the nature of my subject.

Physicists deal very largely with the measurement of extremely minute forces, which it is of the utmost importance that they should be able to measure accurately. Now forces may be considered under two aspects. It may be that the force which is developed and which has to be measured is a twist, in which case the twisting force may be applied to the end of a wire directly, when the amount through which that wire is twisted is a measure of the twisting force. Or the force may be a direct pull

[1] Lecture delivered by Prof. C. Vernon Boys, F.R.S., on September 8, 1890, at the Leeds meeting of the British Association.

or a push, which may also be measured by the twist of a wire if it is applied to the end of a lever or arm carried by the wire.

Now supposing that the force—whether of the nature of a twist or of a pull, it does not matter which—is too small to produce an appreciable twist in the wire, it is obvious that a finer wire must be employed, but it is not obvious how much more easily a fine wire is twisted than a coarse one. If the fine wire is one-tenth of the diameter of the coarse one, we must multiply ten by itself four times over in order to find how much more easily twisted it is, and thus obtain the enormous number 10,000; it is 10,000 times more easily twisted than the coarse one. Thus there is an enormous advantage in increasing the minuteness of the wire by means of which feeble twisting or pulling forces are measured. But if the delicacy of the research is such that even the finest wire which can be made is still too stiff, then, even though with such wire, which is somewhere about the thousandth of an inch in diameter, forces as small as the millionth part of the weight of a single grain can be detected with certainty, the wire is of no use; and as wire cannot be made finer, some other material must be used. Spun glass is fine and strong, and is still more easily twisted than the finest wire, but it possesses a property somewhat analogous to putty. When it has been twisted and then let go, it does not come back to its old place, so that though it is much more largely twisted than wire by the application of a force, it is not possible with accuracy to measure that force. There is, or rather I should say there was, no material that could be used as a torsion thread finer than spun glass; and therefore physicists use instead a fibre almost free from torsion. A single thread of silk as spun by the silkworm is taken and split down the middle, for it is really double, and one half only is used. This is far finer than spun glass, and being softer in texture, it is much more easily twisted. Silk is ten thousand times more easily twisted than spun glass. So easily twisted is silk that in the majority of instruments the stiffness of the silk is either of no consequence at all, or at any rate it only produces but the slightest disturbing effect. Now if it is necessary to push the investigation further still by the continued increase in the delicacy of the apparatus, silk itself begins to prevent any progress. Silk has a certain stiffness, but if that were always the same it would not matter; but then it possesses that putty-like character of spun glass, but in a far higher degree; it is affected by every variation of temperature and moisture, and any really delicate measures are out of the question when silk is used as the suspending fibre.

This, I believe, is a fairly accurate account of the state of the case three years ago. At that time I was improving, or attempting to improve, a certain class of apparatus of which I shall have more to say presently, and I was met by the difficulty that a greater degree of delicacy was required than was possible with existing torsion threads. Silk would have entirely prevented me from reaching the degree of delicacy and certainty in this instrument that I hope to show this evening that I have attained.

Being then in this difficulty, I was by good fortune and necessity led to devise a process which I propose at once to show you. I shall not describe the preliminary experiments, but simply describe the process as it stands. There is a small cross-bow held in a vice, and a little arrow made of straw with a needle point, and I have here a fragment of rock crystal which has been melted and drawn into a rod. It requires a temperature greater than that developed in any furnace to melt this material so that it may be drawn out. If the arrow, which also carries a piece of the quartz rod, is placed in the bow, and if both pieces are heated up to the melting-point and joined together, and then the arrow is shot, a fibre of quartz is drawn—that is to say, it is drawn if there is not an accident.

The arrow has flown, and there is now a fibre, not very fine this time, which I shall hand to our President. At the same time I can pass him a piece of much finer fibre, made this afternoon, which shows (and this is a proof of its fineness) all the brilliant colours of the spider line when the sun shines upon it, but with a degree of magnificence and splendour which has never been seen on any natural object.

The main features of these fibres are these. You can make them as fine as you please; you can make them of very considerable length; you can make pieces 40 or 50 feet long, without the slightest trouble, at almost every shot. Even though of that great length, they are very uniform in diameter from end to end, or, at any rate, the variation is small and perfectly regular. The strength of the fibre is, I think I may safely say, something astonishing. Fibres such as I have in use at the present time in an instrument behind me are stronger than ordinary bar steel: they carry from 60 to 80 tons to the square inch. That is one of their most important features, for this reason—that on account of their enormous strength you can make use of very much finer fibres than would be possible if they were not so strong; and I have already explained the importance of the fineness of the fibre when delicacy is of the first importance.

As to the diameter of these fibres, I have said they can be made as fine as you please. I shall not trouble you with a large number of figures, but one or two may probably be interesting to those who are in the habit of using philosophical apparatus. In the first place, a fibre a great deal finer than a single fibre of silk—that is, one five-thousandth of an inch in diameter—will carry an apparatus more than thirty grains in weight. I have in one of the pieces of apparatus which I shall use presently a fibre the fifteen-thousandth of an inch in diameter. That is, so fine that if you were to take a hundred of them and twist them into a bundle you would produce a compound cable of the thickness of a single silkworm's thread. I do not mean the silk used for sewing that is wound on a reel, because that is composed of an enormous number of silk threads; but a single silkworm's thread as it is wound from the cocoon, and that fibre is at the present time carrying a mirror the movements of which will presently be visible in all parts of this large room.

But that is by no means the limit of the degree of fineness which can be reached. A fibre the fifteen-thousandth of an inch in thickness is quite a strong and conspicuous object. You may go on making them until you cannot see them with the naked eye. You may go on following them with the microscope until you cannot see them with the microscope—that is to say, you cannot find their end—they gradually go out. The ends are so fine that it is impossible ever to see them in any microscope that can be constructed, not because the microscopes are bad, but because of the nature of light. But that is a point upon which I shall not say more this evening. It has been estimated that probably the ends of some of these are as fine as the millionth part of an inch—I do not care whether they are or whether they are not, because they can never be seen and never be used—but certainly the hundred-thousandth of an inch is by no means beyond the limit which can be obtained. As these large numbers of hundreds of thousands and millions are figures which it is impossible for anybody thoroughly to realize, I may for the purpose of illustration say that, if we were to take a piece of quartz about as big as a walnut, and if we could draw the whole of that into a thread one hundred-thousandth of an inch in diameter—threads which can certainly be produced—there would be enough to go round the world about six or seven times.

These quartz fibres, on account of their fineness, are eminently capable of measuring minute forces—that is to say, they would be capable if they were free from that putty-like quality which I have described as making spun glass useless. Now, experiments made both in this country and in Australia show that to a most extraordinary degree they are perfectly free from that one fault of spun glass.

The number of useful properties of quartz that has been melted is so great that I can merely take, in a more or less disjointed way, one or two; and I propose, in the first place, to say something which, I think, may be especially interesting to chemists, and, perhaps, to our President. I should like to ask experimental chemists what they would think of a material which could be drawn into tubes, blown into bulbs, joined together in the same way that glass is joined, drawn out, attached to a Sprengel pump, sealed off with a Sprengel vacuum, which would be transparent, which would be less acted upon than glass by corrosive chemicals, and which, finally, at the point at which platinum is as fluid as water, would still retain its form. Here is such a tube with a bulb blown at the end. I have found that it is possible to make tubes (though it cannot be done in the ordinary way, as with glass) and to blow bulbs with quartz, and that they have this advantage which glass does not possess—namely, that it is almost impossible to crack them by the sudden application of heat.

Then there is another property which quartz fibres and rods possess which I shall be able to show only imperfectly—namely, the power of insulating anything charged with electricity under conditions under which in general insulation is impossible. You now see upon the screen an electroscope, the leaves of which were charged at noon, and they are still divergent, but not to a very great extent, because they have suffered from un-

avoidable shaking during the day. The point to which I specially wish to refer is this. In electroscopes and all electrostatic apparatus one puts in a dish of sulphuric acid, which is an abomination, in order to keep the atmosphere dry. I have in this electroscope such a dish, but it is filled with water in order to keep the atmosphere moist. Experiments carefully made, using the same box—everything the same—except that in one case the insulating stem was made of quartz, and in the second case it was made of the best flint glass, well washed, of the same shape and size, show that, if the atmosphere is perfectly dry, the electricity escapes from both at the same rate; but that, if the atmosphere is perfectly moist, the electricity escapes from the leaves insulated by the clean-washed flint glass only too quickly; whereas, from the leaves insulated by the quartz, the rate is identically the same as it was in either case when the atmosphere was perfectly dry.

I have said that these fibres are uniform in diameter, and fine and smooth and strong, and that they glisten with all the colours of the spider web, but that they are far more brilliant. It was naturally rather a curious point to note what a spider would do if by any chance she should find herself on such a web, and now that I am dealing with live and wild animals which cannot possibly be trained the conditions are such as to render the success of an experiment entirely a matter of chance. However, I propose to make use of the spider as a test of the very great smoothness and slipperiness of one of these fibres. There are here three little spiders which have been good enough, since they came to Leeds, to spin upon these little wooden frames their perfect and beautiful geometrical webs. I have succeeded in placing one of these frames in the lantern without disturbing the spider, which you can now see waiting upon her web. I must now, without disturbing the peace of mind of the spider, carry her to a web of quartz; and therefore it is necessary that the spider should be fortunate enough to catch a fly. Now, instead of bringing a fly I will make an ordinary tuning-fork buzz against the web. She immediately pounces upon the imaginary fly, and thus I can without frightening her place her upon the quartz fibre. Unfortunately this spider has slipped and has got away, but with another I am more successful. I intended to show that the small and common garden spider could not climb the quartz fibre, but for some reason this spider is able to get up with difficulty; however I shall not spend any more time upon this experiment.

I shall now at once speak about the instrument which actually led me to the invention of the process for making quartz fibres. This, which I have called a radio-micrometer, is an instrument of very great delicacy for measuring radiant heat from such a thing as a candle, a fire, the sun, or anything else which radiates heat through space.

The radio-micrometer which I wish to show this evening is resting upon a solid and steady beam, and as usual its index is a spot of light upon the scale. You see that that spot of light is almost perfectly steady. Now the heat that I propose to measure, or rather the influence of which I intend to show you, is the heat which is being radiated from a candle fixed in the front of the upper gallery some 70 or 80 feet from the instrument; and in order that you may be sure that the indication of the instrument is due to the heat from the candle, and not to any manipulation of the apparatus on the beam, I shall perform the experiment as follows. None of the apparatus at this end of the room will be touched or moved in any way; but by a string I shall simply pull the candle along a slide up to a stop, at which position it will shine upon the sensitive part of the radio-micrometer. Instantly the spot of light darts along the scale for a distance of ten feet, and then after leaving the scale it comes to rest upon the face of the balcony five or six seconds after it began to move. Now if the candle is allowed to move back through about a foot, you will see that the instrument will cool down at once—it is at present suffering from the heat which falls upon it from the distant candle; but it will cool down at once, and the index will go back to its old place. It is very nearly at its old place now. I will now let the candle shine upon it again. The index at once goes on to the balcony as before, and now that the candle is moved away again, the index has assumed its old place upon the scale.

That really shows that we have here the means of measuring heat with a degree of delicacy, and also with a degree of certainty, ease, and quickness, which has never yet been equalled. It is probable that the measure which I have given of the degree of delicacy that I have reached in my astronomical apparatus—namely, that the heat of a candle more than two miles away can certainly be felt—will not seem so absurd now that you have seen this less perfect apparatus at work, as it does to people whose experience is limited by the thermopile or their senses.

You can now see the spot of light; it is perfectly quiet in its old place. I wish to show you that this instrument is unlike those which are ordinarily used for this purpose. All the heat, the very considerable heat, due to this electric arc lamp, is actually falling on the instrument, but not upon its sensitive surface, and there is no indication. There are a large number of people in the room—it does not feel the heat from them. Stray heat which it is not meant to feel—which is not in the line along which it can see, or feel—has no influence upon it. When the candle was moved to the place to which it was looking, it felt the heat, and you saw the movement of the index. What is perhaps more important than all is, that it is an instrument which does not even feel the influence of a magnet. I have here a magnet, and on waving the magnet about near the instrument there is no movement of the index at all; it does not dance up and down the scale, as it certainly would do in the case of a galvanometer, because this magnet would affect a galvanometer at the other end of the room. We have then a degree of sensibility which is certainly not easily developed in any other way. I must except, however, the instrument which Prof. Langley of America has recently brought to a great state of perfection. I am unable to state, from want of information, whether his instrument is as sensitive as the one I have just shown, but whether it is or is not as sensitive it certainly cannot compare with this in its freedom from the disturbing effects of stray heat falling upon it, or of the magnetic or thermo-electric disturbances which give so much trouble where the galvanometer is employed.

Now this apparatus I was recently using in some astronomical experiments on the heat of the moon and the stars. As these experiments could only be made with an instrument such as this, possessing extreme sensibility and freedom from extraneous disturbances, and as this instrument is both the cause of the discovery and the first result of the application of quartz fibres, I have thought it well to repeat a typical experiment upon the moon's heat, but, like Peter Quince, I am in this difficulty. As he said, "There is two hard things, that is to bring the moonlight into a chamber." In fact, at the present time the moon has not risen, and if it had we should not be much better off. Peter Quince proposed that they should in case of moonlight failing have a lanthorn and a bunch of thorns. That no doubt was sufficient for the conversation of Pyramus and Thisbe, but that would not do for the purpose of showing the variation of radiation from point to point upon the moon's surface, and as that is the experiment which I now wish to show—an experiment which this instrument enables one to make with the greatest ease and certainty—it is necessary to have something better than a lanthorn and a bunch of thorns. Therefore I have been obliged, as the moon is not available, to bring a moon. Now this moon is a real moon; it is not a representation; it is not a slide; it is a real moon, and it is made by taking an egg-shell and painting it white. That egg-shell is now placed upon a stand, and is illuminated by the sun—that is, an electric light—and in order that the moon may be visible the room must be darkened. The moon is now shining in the sky. An image of the moon is cast by means of a concave mirror upon a translucent screen. There is in addition another mirror which throws a small image of the same moon upon the radio-micrometer. There is one more thing to explain. There is upon the screen a black spot which represents the sensitive surface of the radio-micrometer. That bears the same proportion to the moon which you see on the screen as the sensitive surface of the radio-micrometer bears to the image of the moon that is cast upon it. Now the two mirrors are arranged to move by clockwork, so as to make the two images travel at proportional rates. The moon is travelling with the dark edge foremost, and now that the terminator of the moon has come upon the sensitive surface, the heat is felt and the deflection of the instrument is the result. Now as the moon is gradually travelling through the sky, the radiation is slowly and steadily increasing, because the radiation from the moon gets greater and greater, as the point at which the sun is shining vertically—that is, a point at right angles with the terminator—is approached; it is here a maximum, and then it falls back, and as soon as the moon has gone off the instrument, you will see the index fall back almost suddenly. But there is something more. This moon in one respect is better than the other moon. At the present time it represents the moon nineteen days old, a moon, that is to say, which is waning, and which goes through the sky

with its dark edge foremost. The clockwork will now bring the moon back again, and convert the nineteen-day moon into a nine-day moon, one in which the bright edge goes forward. What I want you to notice, and it will be perfectly evident, is this, that the spot of light will now go up the scale suddenly, will then rise to a maximum position, and will then fall slowly until the terminator is reached, which proves that in the former case the slow rise and sudden fall, or the present sudden rise and slow fall was not a peculiarity of the instrument, but was due to the fact that the different points of the moon radiated in the manner which I have stated. There is one point which, as the moon has now left the instrument, I should like to show; that is, that it is a real moon and not a mere slide. That is shown by gradually moving the sun round. Now it is at right angles to the line of view, and we have got the half moon. As it goes round, the moon continues waning, appearing more like a new moon, and at last we have an eclipse of the sun, which may be annular if the proportions of the apparatus are properly arranged.

I wish now to make a few statements as to the delicacy of apparatus that can be made with the help of quartz fibres. I would wish you most distinctly to understand that it is not sufficient to go into a shop and buy apparatus as it is now made, replace the silk by quartz, and to suppose you can get a degree of delicacy such as I have shown you. That is not sufficient. If you take out the silk and put in a quartz fibre the apparatus will be much improved, and you can then increase its delicacy. You will then escape the troubles due to silk; but one after the other a new series of disturbances will appear, and anything like ultimate, extreme and minute accuracy will still seem out of the question. Now it has been my business to eliminate one by one these disturbing influences. I will not weary you with a description of them all, and the methods by which they may be certainly provided against. These disturbing causes, which at the present time with instruments carrying a silk fibre are not even known to exist, or if known to exist, are practically of no consequence whatever, come one by one into prominence, when you attempt to push the delicacy of your apparatus to the extent that I have reached in the home-made apparatus which I have here this evening. I do not propose to give more than one illustration, and as this is one which I found out by accident, and which at the time very much annoyed me, I imagine that it may be of interest to explain the circumstances under which this was observed.

In the experiments I made on the heat of the moon and the stars it was necessary to determine to what degree of delicacy the apparatus could be brought—that is to say, to determine what deflection would be produced by a known and familiar source of radiation. For this purpose the source of heat that I used was a common candle, placed sufficiently far off to produce a convenient deflection. I began by placing the candle about 100 yards away, but I was obliged to place the candle at a distance of 250 yards. At that distance I could not conveniently at night turn the shutter on and off with a string. Therefore I adopted the more simple and practical plan of asking my niece to stand at the top of the hill and to pull the string when I gave the signal. The signal was nothing more nor less than my saying the word "on" or "off," so that without moving I could observe the deflection due to the heat of the candle at that distance. Those were the circumstances, but when I shouted "on," before the sound could have reached my niece at the top of the hill, the spot of light had been driven violently off the scale. This seemed as if, as I suspected at the time, one of my little eight-legged friends had got inside the apparatus, and feeling the trembling due to the sound, struck forward, as the diadema spider is known to do, and tried to catch the thing that was flying by. But further experiments showed that this was not the case. It happened that the sound of my voice was just that to which the telescope tube would respond. It echoed to that note, the instrument felt the vibration of the air, and that was the result.

In order to show that an instrument will feel the motion in the air under the influence of sound, I have arranged an experiment of the simplest possible character. I should say that the first instrument of this kind was made many years ago by Lord Rayleigh; but I feel sure that even he would not be prepared for the delicacy to which apparatus on this principle can be brought. It simply depends upon this familiar and well-known fact. A card or a leaf allowed to drop through the air does not fall the way of the least resistance—that is, edgeways—but it turns into the position of greatest resistance, and falls broadside on, or it overshoots the mark, and so gets up a spin.

Supposing you take a little mirror suspended at an angle of 45° to the direction of the waves of sound, the instant sound-waves proceed to travel, that mirror turns so as to get into such a position as to obstruct them. The mirror that I have for this purpose weighs about the twentieth part of a grain, and the fibre on which it is suspended is about the fifteen-thousandth part of an inch in diameter. The mirror is so small and light that the moment of inertia is a two-hundredth part of that which people ordinarily call the minute and delicate needle of the Thomson mirror-galvanometer. With a fibre only a few inches long, there is no difficulty in getting a period of oscillation of ten or eleven seconds. When the light from the lamp is reflected and falls upon the scale, as it will be in a minute, then a movement of the light from one of those great divisions to the next—that is, a movement of three inches—will correspond to a twisting force such as would be produced by pulling the end of a lever an inch long with a force of a thousand-millionth part of the weight of a grain. It would be easy to observe a movement ten or a hundred times less. My difficulty now is that it is impossible to speak and at the same time to keep that spot at rest, because the instrument is arranged to respond to a certain note. This is not the predominating note of my voice, but since the voice, like all other noises as distinguished from pure musical sounds, consists of a great number of notes, every now and then the note to which the instrument is tuned is sure to be sounded, and then it will respond. Therefore, while I am speaking it is impossible to keep the spot of light at rest. However, in order to show that the instrument does respond to certain notes, even if feeble, with a degree of energy and suddenness which I believe would never be expected, I shall with these small organ pipes sound three notes. But I must explain beforehand what I am going to do, as the sound of my voice will spoil the experiment. I shall, standing as far away as I can get from the instrument, first sound a note that is too high; I shall then sound a note that is too low; and then I shall sound the note to which the instrument is tuned. I must ask everyone during this experiment to be as quiet as possible, as the faintest sound of the right sort will interfere with the success of the experiment. The first two notes sounded loudly produced no result, while the moment the right note was heard the light went violently off the scale and travelled round the room. When this little organ pipe was blown at the farthest end of the room this afternoon, it drove the light off the scale, almost as violently as it did just now.

[The Cavendish experiment of observing the attraction due to gravitation between masses of lead was then explained; and the actual experiment, performed with apparatus no larger than a galvanometer, in which the attracting masses were two pounds and fifteen grains respectively, in which the beam was only about five-eighths of an inch long, and in which the total force was less than one ten-millionth of the weight of a grain, was then shown. The actual deflection on the scale was rather more than ten feet, and eighty seconds were required for the single oscillation. With this apparatus forces two thousand times as small could be observed, though the fibre is, in comparison with others that were made use of, exceedingly coarse. Forces equivalent to one million-millionth of the weight of a grain were stated to be within the reach of a manageable quartz fibre.]

Now that I have shown all that my limited time has permitted me, I wish finally to answer a question which is frequently put to me, and which possibly some in the room may have asked themselves. The question may be put broadly in this form: "These fibres no doubt are very fine, and very wonderful, but are they of any practical use?" This is a question which I find it difficult to answer, because I do not clearly know what is meant by "practical use." If by "a thing of practical use" you mean something which is good to eat or to drink, or if you mean something which we may employ to protect ourselves from the extremes of heat or cold or moisture, or if you mean—and this is a point which those who have studied biology will perhaps appreciate more than others—something which may be made use of for the purpose of personal adornment; if that is what you mean by "practical use," then with the exception of the possibility of being able to weave garments of an extraordinary degree of fineness, softness, and transparency, quartz fibres are of no "practical use." But if you mean something which will enable a large and distinguished body of men to do that which is most important to them more perfectly than has been possible hitherto—I allude of course to the experimental philosopher and his experimental work, which after all has laid the foundations upon which so much that is called practical

actually is built—if this is what you mean, then I hope that the few experiments which I have been able to show this evening are sufficient to prove that quartz fibres are of some practical use; and they have served this additional purpose—with what success I am unable to say—they have provided a subject for an evening lecture of the British Association.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, October 1.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The Rev. Dr. Walker exhibited, and read notes on, a long and varied series of *Crymodes exulis*, collected in June and July last in Iceland. In reply to a question by Lord Walsingham as to whether all the forms referred by Dr. Walker to *Crymodes exulis* had been identified as belonging to that species, Mr. Kirby said the species was a very variable one, and that several forms had been described from Labrador and Greenland. Mr. South stated he believed that most of the forms had been described by Dr. Staudinger.—Dr. D. Sharp, F.R.S., exhibited a specimen of *Ornithomyia avicularia*, L., taken near Dartford, to which there were firmly adhering—apparently by their mandibles—several specimens of a mallophagous insect. He also exhibited specimens of fragile Diptera, Neuroptera, and Lepidoptera, to show that the terminal segments in both sexes might be dissected off and mounted separately without the structures suffering from shrivelling or distortion.—Mr. G. F. Hampson exhibited a series of *Erebia melas*, taken in July last, in the Austrian Alps (Dolomites), by Mrs. Nicholls. Captain Elwes observed that this species was abundant in the Pyrenees, but he had never been able to obtain specimens from any other part of Europe; and that it had been left to an English lady to first take a species of *Erebia* new to these Alps. He added that the species only frequented very steep and stony slopes on the mountains, so that its capture was attended with difficulty.—Mr. McLachlan, F.R.S., exhibited specimens of an extraordinary Neuropterous larva found by Mr. B. G. Nevinson in tombs at Cairo. He said that this larva had been assigned to the genus *Nemoptera* by Schaum, and Roux had previously described and figured it as an abnormal apterous hexapod under the name of *Necrophilus arenarius*. Mr. Nevinson supplemented these remarks with an account of his capture of the specimens in the Egyptian tombs.—Mr. G. T. Baker exhibited species of the genus *Boarmia* from Madeira; and also melanic varieties of *Gracilaria syringella* from the neighbourhood of Birmingham.—Mr. W. F. H. Blandford exhibited and remarked on specimens of *Dermestes vulpinus*, a wood-boring beetle, which had been doing much damage to the roofs of certain soap-works in the neighbourhood of London.—Mr. R. W. Lloyd exhibited a specimen of *Carabus catenulatus*, in which the femur of the right foreleg was curiously dilated and toothed.—The Rev. C. F. Thornewill exhibited a black variety of the male of *Argynnis aglaia*, taken by himself in July last on Cannock Chase; also a number of living larvæ of a species of *Eupithecia* feeding on the flower-heads of *Tanacetum vulgare*. He expressed some doubt as to the identity of the species, but the general opinion was that the larvæ were those of *Eupithecia absynthiata*.—Mr. H. Goss exhibited, for Mr. G. Bryant, a variety of the larva of *Trichiura cratægi*.—Mr. C. G. Barrett exhibited a specimen of *Plusia moneta*, Fabr., a species new to Britain, taken at Reading in July last. Mr. Goss stated that the first specimen of this species had been taken at Dover last June, and was now in the collection of Mr. Sydney Webb, of that town. Mr. Kirby said that Mynheer Snellen had reported this species as being unusually common in Holland a few years ago.—Mr. W. Dannatt exhibited a variety of *Papilio hectorides* from Paraguay. Mr. O. Salvin, F.R.S., said he had seen this form before.—Mr. C. J. Gahan exhibited a curious little larva-like creature, found in a mountain stream in Ceylon, and observed that there was some doubt as to its true position in the animal kingdom. It was made up of six distinct segments, each of which bore a single pair of laterally directed processes or unjointed appendages. Mr. Hampson remarked that the appendages were very suggestive of the parapodia of certain chætopod worms. Lord Walsingham and Mr. McLachlan expressed an opinion that the animal was of myriopodous affinities, and was not the larva of an insect.—Mr. Baker read a paper entitled "Notes on the genitalia of a gynandromorphous *Eronia hippia*."

Paris.

Academy of Sciences, October 6.—M. Duchartre in the chair.—On the determination of integrals of certain equations from partial derivatives of the second order, by M. Émile Picard.—On the balls of fire or electric globes of the St. Claude tornado, according to the report of M. Cadenat, by M. H. Faye. Prof. Cadenat, of the St. Claude College, has brought forward a number of testimonies as to the appearance of many balls of fire during the storm of August 19. It is a remarkable fact that the United States tornadoes are rarely accompanied by globular lightning discharges like those observed during the recent storms of Dreux or St. Claude. The cause may be that American tornadoes have been most frequently observed in broad daylight, whilst in France those of August 18 and 19 appeared towards the evening.—On the movement of Foucault's pendulum, by M. de Sparre. The author establishes the complete formulæ for the movement of Foucault's pendulum in air, and shows that the resistance of the air has an indirect influence on the velocity of rotation of the plane of oscillation, both diminishing the amplitude of the vibrations, and causing deformations in the curve described.—Some theorems on similar plane figures, by M. P. H. Schoute.—On a new method for testing urea, by M. M. P. Miquel.—Destruction of the tubercular virus by the products of the evaporation of certain substances, such as a mixture of alcohol and different essences, on spongy platinum, by M. Onimus.—On the fecundation of *Hydatina senta*, Ehr., by M. Maupas.—Experiments on the cultivation of wheat in a sterile siliceous soil, by M. Pagnoul. The experiments show that phosphates, especially in the soluble form, play an important *rôle* in the production of wheat; in fact, the suppression of phosphoric acid retarded the maturity of plants about ten days. The richness of the grain in nitrogenous matters increases with the proportion of nitrogen at the disposal of the plant. It it is found to decrease to 8 or 9 per cent in plants grown in soil containing no nitrogen, and reaches as much as 20 per cent—that is, much above the average—in those grown in soils in which the assimilated nitrogen was greater than that of the most fertile soils.—Observations of the part played by fluor in mineralogical syntheses, by M. Stanislas Meunier. The author finds that the introduction of fluorides renders the synthesis of labrador, nephelite, and leucite remarkably easy and rapid, and does away with the necessity for very high temperatures.

CONTENTS.

o

in the
ations
Emile
Claude
Faye.
rward
lls of
t that
bular
torms
erican
light,
wards
m, by
læ for
s that
locity
g the
n the
es, by
by M.
y the
as a
inum,
Ehr.,
t in a
show
ortant
ion of
days.
s with
t. It
wn in
o per
n soils
of the
uor in
author
esis of
d, and